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LETTERS

OF

EULER.

LETTERS

OF

EULER

ON DIFFERENT SUBJECTS

IN

PHYSICS AND PHILOSOPHY.

ADDRESSED TO

A GERMAN PRINCESS.

TRANSLATED FROM THE FRENCH BY

HENRY HUNTER, D.D.

WITH

ORIGINAL NOTES,

And a Gloſſary of Foreign and Scientific Terms:

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LETTERS

LETTERS
ON
DIFFERENT SUBJECTS
IN
PHYSICS AND PHILOSOPHY.

LETTER I.

Continuation of the Subject, and of Miſtakes in the Knowledge of Truth.

MADAM,

THE three claſſes of truths which I have now unfolded, are the only ſources of all our knowledge; all being derived from our own experience, from reaſoning, or from the report of others.

It is not eaſy to determine which of theſe three ſources contributes moſt to the increaſe of knowledge. Adam and Eve muſt have derived theirs chiefly from the two firſt; God, however, revealed many things to them, the knowledge of which is to be referred to the third ſource, as neither their own experience, nor their powers of reaſoning, could have conducted them ſo far.

Without recurring to a period ſo remote, we are ſufficiently convinced, that if we were determined to believ [illegible] of what we hear from others, or read in their writings, we ſhould be in a ſtate of almoſt total ignorance. It is very far, however, from being our duty to believe every thing that is ſaid, or that we read. We ought conſtantly to employ our diſcerning faculties, not only with reſpect to truths of the third claſs, but likewiſe of the two others.

We are ſo liable to ſuffer ourſelves to be dazzled by the ſenſes, and to miſtake in our reaſonings, that the very ſources laid open by the Creator for the diſcovery of truth, very frequently plunge us into error. Notions of the third claſs, therefore, ought not, in reaſon, to fall under ſuſpicion, any more than ſuch as belong to the other two. We ought, therefore, to be equally on our guard againſt deception, whatever be the claſs to which the notion belongs; for we find as many inſtances of error in the firſt and ſecond claſſes as in the third. The ſame thing holds with regard to the certainty of the particular articles of knowledge which theſe three ſources ſupply; and it cannot be affirmed that the truths of any one order have a ſurer foundation than thoſe of another. Each claſs is liable to errors, by which we may be miſled; but there are likewiſe precautions which, carefully obſerved, furniſh us with nearly the ſame degree of conviction. I do not know whether you are more thoroughly convinced of this truth, that two triangles which have the ſame baſe, and the ſame height, are equal to one another, than of this, that the Ruſſians have been at Berlin; though the former is founded on a chain of accurate reaſoning, whereas the latter depends entirely on the veracity of your informer.

Reſpecting the truths, therefore, of each of theſe claſſes, we muſt reſt ſatisfied with ſuch proofs as correſpond to their nature; and it would be ridiculous to inſiſt upon a geometrical demonſtration of the truths of experience, or of hiſtory. This is uſually the fault of thoſe who make a bad uſe of their penetration in intellectual truths, to require mathematical demonſtration, in proof of all the truths of religion, a great part of which belongs to the third claſs.

There are perſons determined to believe and admit nothing but what they ſee and touch; whatever you would prove to them by reaſoning, be it ever ſo ſolid, they are diſpoſed to ſuſpect, unleſs you place it before their eyes. Chymiſts, anatomiſts, and natural philoſophers, who employ themſelves wholly in making experiments, are moſt chargeable with this fault. Every thing that the one cannot melt in his crucible, or the other diſſect with his ſcalpel, they reject as unfounded. To no purpoſe you would ſpeak to them of the qualities and nature of the ſoul; they admit nothing but what ſtrikes the ſenſes.

Thus, the particular kind of ſtudy to which every one is addicted, has ſuch a powerful influence on his manner of thinking, that the natural philoſopher and chymiſt will have nothing but experiments, and the geometrician and logician nothing but arguments; which conſtitute, however, proofs entirely

different,

different, the one attached to the firſt claſs, the other to the ſecond, which ought always to be carefully diſtinguiſhed, according to the nature of the objects.

But can it be poſſible that perſons ſhould exiſt, who, wholly abſorbed in purſuits pertaining to the third claſs, call only for proofs derived from that ſource? I have known ſome of this deſcription, who, totally devoted to the ſtudy of hiſtory and antiquity, would admit nothing as true, but what you could prove by hiſtory, or the authority of ſome ancient author. They perfectly agree with you, reſpecting the truth of the propoſitions of Euclid, but merely on the authority of that author, without paying any attention to the demonſtrations by which he ſupports them; they even imagine that the contrary of theſe propoſitions might be true, if the ancient geometricians had thought proper to maintain it.

This is a ſource of error which retards many in the purſuit of truth; but we find it rather among the learned, than among thoſe who are beginning to apply themſelves to the ſtudy of the ſciences. We ought to have no predilection in favour of any one of the three ſpecies of proofs which each claſs requires; and provided they are ſufficient, in their kind, we are bound to admit them.

I have ſeen or felt, is the proof of the firſt claſs. *I can demonſtrate it*, is that of the ſecond; we likewiſe ſay, *I know it is ſo.* Finally, *I receive it on the teſtimony of perſons worthy of credit*, or *I believe it on ſolid grounds*, is the proof of the third claſs.

4th April, 1761.

LETTER II.

Firſt Claſs of known Truths. Conviction that Things exiſt externally correſponding to the Ideas repreſented by the Senſes. Objection of the Pyrrhoniſts. Reply.

WE include in the firſt claſs of known truths, thoſe which we acquire immediately by means of the ſenſes. I have already remarked, that they not only ſupply the ſoul with certain repreſentations relative to the changes produced in a part of the brain; but that they excite there a conviction of the real exiſtence of things external, correſponding to the ideas which the ſenſes preſent to us.

The ſoul is frequently compared to a man ſhut up in a dark room, in which the images of external objects are repreſented on the wall by means of a glaſs. This compariſon is tolerably juſt, as far as it reſpects the man looking at the images on the wall; for this act is ſufficiently ſimilar to that of the ſoul, contemplating the impreſſions made in the brain; but the compariſon appears to me extremely defective, as far as it reſpects the conviction, that the objects, which occaſion theſe images, really exiſt.

The man in the dark room will immediately ſuſpect the exiſtence of theſe objects; and, if he has no doubt about the matter, it is becauſe he has been out of doors, and has ſeen them; beſides this, knowing the nature of his glaſs, he is aſſured, that nothing can be repreſented on the wall but the images of the objects which are without the chamber before the glaſs.

But this is not the caſe with the ſoul; it has never quitted its place of reſidence to contemplate the objects themſelves: and it knows ſtill leſs the conſtruction of the ſenſitive organs, and the nerves which terminate in the brain. It is, nevertheleſs, much more powerfully convinced of the real exiſtence of objects, than our man in the dark room poſſibly can be. I am apprehenſive of no objection on the ſubject, the thing being too clear of itſelf to admit any, though we do not know the true foundation of it. No one ever entertained any doubt about it, except certain viſionaries, who have bewildered themſelves in their own reveries. Though they ſay, that they doubt the exiſtence of external objects, they entertain no ſuch doubt in fact; for why would they have affirmed it, unleſs they had believed the exiſtence of other men, to whom they wiſhed to communicate their extravagant opinions?

This conviction, reſpecting the exiſtence of the things whoſe images the ſenſes repreſent, appears not only in men of every age and condition, but likewiſe in all animals. The dog which barks at me has no doubt of my exiſtence, though his ſoul perceives but a ſlight image of my perſon. Hence I conclude, that this conviction is eſſentially connected with our ſenſations, and that the truths which the ſenſes convey to us, are as well founded as the moſt undoubted truths of geometry.

Without this conviction no human ſociety could ſubſiſt, for we ſhould be continually falling into the greateſt abſurdities, and the groſſeſt cont s.

Were the peaſantry to dream of doubting about the exiſtence of their bailiff, or ſoldiers about that of their officers, into what confuſion ſhould we be plunged! ſuch abſurdities are entertained only by philoſophers; any other giving himſelf up to them, would be conſidered as having loſt his reaſon. Let us, then, acknowledge this conviction as one of the principal laws of nature, and that it is complete, though we are abſolutely ignorant of its true reaſons, and very far from being able to explain them in an intelligible manner.

However important this reflection may be, it is by no means, however, exempted from difficulties; but were they ever ſo great, and though it might be impoſſible for us to ſolve them, they do not in the ſmalleſt degree affect the truth which I have juſt eſtabliſhed, and which we ought to conſider as the moſt ſolid foundation of human knowledge.

It muſt be allowed, that our ſenſes ſometimes deceive us; and hence it is that thoſe ſubtile philoſophers, who value themſelves on doubting of every thing, deduce the conſequence, that we ought never to depend on our ſenſes. I have perhaps oftener than once met an unknown perſon in the ſtreet, whom I miſtook for an acquaintance: as I was deceived in that inſtance, nothing prevents my being always deceived; and I am, therefore, never aſſured, that the perſon to whom I ſpeak is in reality the one I imagine.

Were I to go to Magdeburg, and to preſent myſelf to your Highneſs, I ought always to be apprehenſive of groſsly miſtaking: nay, perhaps, I ſhould not be at Magdeburg, for there are inſtances of a man's ſometimes taking one city for another. It is even poſſible I may never have had the happineſs of ſeeing you, but was always under the power of deluſion, when I thought myſelf to be enjoying that felicity.

Such are the natural conſequences reſulting from the ſentiments of certain philoſophers; and you muſt be abundantly ſenſible, that they not only lead to manifeſt abſurdity, but have a tendency to diſſolve all the bands of ſociety.

7th April, 1761.

LETTER III.

Another Objection of the Pyrrhoniſts againſt the Certainty of Truths perceived by the Senſes. Reply; and Precautions for attaining Aſſurance of ſenſible Truths.

THOUGH the objection raiſed againſt the certainty of truths perceived by the ſenſes, of which I have been ſpeaking, may appear ſufficiently powerful, attempts have been made to give it additional ſupport from the well-known maxim, that we ought never to truſt him who has once deceived us. A ſingle example, therefore, of miſtake in the ſenſes, is ſufficient to deſtroy all their credit. If this objection is well-founded, it muſt be admitted, that human ſociety is, of courſe, completely ſubverted.

By way of reply, I remark, that the two other ſources of knowledge are ſubject to difficulties of a ſimilar nature, nay perhaps ſtill more formidable. How often are our reaſonings erroneous? I venture to affirm, that we are much more frequently deceived by theſe, than by our ſenſes. But does it follow, that our reaſonings are always fallacious, and that we can have no dependance on any truth diſcovered to us by the underſtanding? It muſt be a matter of doubt, then, whether two and two make four, or whether the three angles of a triangle be equal to two right angles; it would even be ridiculous to pretend that this ſhould paſs for truth. Though, therefore, men may have frequently reaſoned inconcluſively, it would be moſt abſurd to infer, that there are not many intellectual truths, of which we have the moſt complete conviction.

The ſame remark applies to the third ſource of human knowledge, which is unqueſtionably the moſt ſubject to error. How often have we been deceived by a groundleſs rumour, or falſe report reſpecting certain events? And who would be ſo weak as to believe all that gazetteers and hiſtorians have written? At the ſame time, whoever ſhould think of maintaining that every thing related or written by others is falſe, would undoubtedly fall into greater abſurdities, than the perſon who believed every thing. Accordingly, notwithſtanding ſo many groundleſs reports and falſe teſtimonies, we are perfectly aſſured of the truth of numberleſs facts, of which we have no evidence but teſtimony.

There are haracters which enable us to

diſtinguiſh truth, and each of the three ſources has characters peculiar to itſelf. When my eyes have deceived me, in miſtaking one man for another, I preſently diſcovered my error; it is evident, therefore, that precautions may be uſed for the prevention of error. If there were not, it would be impoſſible ever to perceive that we had been deceived. Thoſe, then, who maintain that we ſo often deceive ourſelves, are obliged to admit, that it is poſſible for us to perceive we have been deceived, or they muſt acknowledge that they themſelves are deceived when they charge us with error.

It is remarkable, that truth is ſo well eſtabliſhed, that the moſt violent propenſity to doubt of every thing, muſt come to this, in ſpite of itſelf. Therefore, as logick preſcribes rules for juſt reaſoning, the obſervance of which will ſecure us from error, where intellectual truth is concerned; there are likewiſe certain rules, as well for the firſt ſource, that of our ſenſes, as for the third, that of belief.

The rules of the firſt are ſo natural to us, that all men, the moſt ſtupid not excepted, underſtand and practiſe them much better than the greateſt ſcholars are able to deſcribe them. Though it may be eaſy ſometimes to confound a clown, yet when the hail deſtroys his crop, or the thunder breaks upon his cottage, the moſt ingenious philoſopher will never perſuade him that it was a mere illuſion; and every man of ſenſe muſt admit that the country-fellow is in the right, and that he is not always the dupe of the fallacious̄neſs of his ſenſes. The philoſopher may be able, perhaps, to perplex him to ſuch a degree that he ſhall be unable to reply, but he will inwardly treat all the fine reaſonings, which attempted to confound him, with the utmoſt ſcorn. The argument, that the ſenſes ſometimes deceive us, will make but a very ſlight impreſſion on his mind; and when he is told, with the greateſt eloquence, that every thing the ſenſes repreſent to us has no more reality than the viſions of the night, it will only provoke laughter.

But if the clown ſhould pretend to play the philoſopher, in his turn, and maintain that the bailiff is a mere phantom, and that all who conſider him as ſomething real, and ſubmit to his authority, are fools; this ſublime philoſophy would be in a moment overturned, and the leader of the ſect ſoon made to feel, to his coſt, the force of the proofs which the bailiff could give him of the reality of his exiſtence.

You muſt be perfectly ſatisfied, then, that there are certain characters which deſtroy every ſhadow of doubt reſpecting the reality and truth of what we know by the ſenſes; and theſe ſame characters are ſo well known, and ſo ſtrongly impreſſed on our minds, that we are never deceived when we employ the precautions neceſſary to that effect. But it is extremely difficult to make an exact enumeration of theſe characters, and to explain their nature. We commonly ſay, that the ſenſitive organs ought to be in a good natural ſtate; that the air ought not to be obſcured by a fog; finally, that we muſt employ a ſufficient degree of attention, and endeavour, above all things, to examine the ſame object by two or more of our ſenſes at once. But I am firmly perſuaded, that every one knows, and puts in practice, rules much more ſolid than any which could be preſcribed to him.

11th April, 1761.

LETTER IV.

Of demonſtrative, phyſical, and particularly of moral, Certainty.

THERE are, therefore, three ſpecies of knowledge, which we muſt conſider as equally certain, provided we employ the precautions neceſſary to ſecure us againſt error. And hence, likewiſe, reſult three ſpecies of certainty.

The firſt is called *phyſical certainty*. When I am convinced of the truth of any thing, becauſe I myſelf have ſeen it, I have a phyſical certainty of it; and, if I am aſked the reaſon, I anſwer, that my own ſenſes give me full aſſurance of it, and that I am, or have been, an eye-witneſs of it. It is thus I know, that Auſtrians have been at Berlin, and that ſome of them committed great irregularities there. I know, in the ſame manner, that fire conſumes all combuſtible ſubſtances; for I myſelf have ſeen it, and I have a phyſical certainty of its truth.

The certainty which we acquire by a proceſs of reaſoning, is called *logical* or *demonſtrative certainty*, becauſe we are convinced of its truth by demonſtration.

tion. The truths of geometry may here be produced as examples, and it is logical certainty which gives us the aſſurance of them.

Finally, the certainty which we have of the truth of what we know only by the report of others, is called *moral certainty*, becauſe it is founded on the *credibility* of the perſons who make the report. Thus, you have only a moral certainty that the Ruſſians have been at Berlin, and the ſame thing applies to all hiſtorical facts. We know with a moral certainty, that there was formerly at Rome a Julius Cæſar, an Auguſtus, a Nero, &c. and the teſtimonies reſpecting theſe are ſo authentic, that we are as fully convinced of them, as of the truths which we diſcover by our ſenſes, or by a chain of fair reaſoning.

We muſt take care, however, not to confound theſe three ſpecies of certainty, phyſical, logical, and moral, each of which is of a nature totally different from the others. I propoſe to treat of each ſeparately; and ſhall begin with a more particular explanation of moral certainty, which is the third ſpecies.

It is to be attentively remarked, that this third ſource divides into two branches, according as others ſimply relate what they themſelves have ſeen, or made full proof of by their ſenſes, or as they communicate to us, together with theſe, their reflections and reaſonings upon them. We might add ſtill a third branch, when they relate what they have heard from others.

As to this third branch, it is generally allowed to be very liable to error, and that a witneſs is to be believed only reſpecting what he himſelf has ſeen or experienced. Accordingly, in courts of juſtice, when witneſſes are examined, great care is taken to diſtinguiſh, in their declarations, what they themſelves have ſeen and experienced, from what they frequently add of their reflections and reaſonings upon it. Streſs is laid only on what they themſelves have ſeen or experienced; but their reflections, and the concluſions which they draw, however well founded they may otherwiſe be, are entirely ſet aſide. The ſame maxim is obſerved with reſpect to hiſtorians, and we wiſh them to relate only what they themſelves have witneſſed, without purſuing the reflections which they ſo frequently annex, though theſe may be a great ornament to hiſtory. Thus we have a greater dependance on the truth of what others have experienced by their own ſenſes, than on what they have diſcovered by purſuing their meditations. Every one wiſhes to be maſter of his own judgment, and unleſs he himſelf feels the foundation and the demonſtration, he is not perſuaded.

Euclid would in vain have announced to us the moſt important truths of geometry; we ſhould never have believed him on his word, but have inſiſted on proſecuting the demonſtration ſtep by ſtep ourſelves. If I were to tell you, that I had ſeen ſuch or ſuch a thing, ſuppoſing my report faithful, you would without heſitation give credit to it; nay I ſhould be very much mortified if you were to ſuſpect me of falſhood. But when I inform you, that in a right-angled triangle, the ſquares deſcribed on the two ſmaller ſides are together equal to the ſquare of the greater ſide, I do not wiſh to be believed on my word, though I am as much convinced of it, as it is poſſible to be of any thing; and though I could allege, to the ſame purpoſe, the authority of the greateſt geniuſes who have had the ſame conviction, I ſhould rather wiſh you to diſcredit my aſſertion, and to withhold your aſſent, till you yourſelf comprehended the ſolidity of the reaſonings on which the demonſtration is founded.

It does not follow, however, that phyſical certainty, or that which the ſenſes ſupply, is greater than logical certainty, founded on reaſoning; but, whenever a truth of this ſpecies preſents itſelf, it is proper that the mind ſhould give cloſe application to it, and become maſter of the demonſtration. This is the beſt method of cultivating the ſciences, and of carrying them to the higheſt degree of perfection.

The truths of the ſenſes, and of hiſtory, greatly multiply the particulars of human knowledge; but the faculties of the mind are put in action only by reflection or reaſoning.

We never ſtop at the ſimple evidence of the ſenſes, or the facts related by others; but always follow them up and blend them with reflections of our own: we inſenſibly ſupply what ſeems deficient, by the addition of cauſes and motives, and the deduction of conſequences. It is extremely difficult, for this reaſon, in courts of juſtice, to procure ſimple, unblended teſtimony, ſuch as contains what the witneſs actually ſaw and felt, and no more; for witneſſes ever will be mingling their own reflections, without perceiving that they are doing ſo.

14th April, 1761.

LETTER V.

Remarks that the Senſes contribute to the Increaſe of Knowledge; and Precautions for acquiring the Certainty of Hiſtorical Truths.

THE knowledge ſupplied by our ſenſes is undoubtedly the earlieſt which we acquire, and upon this the ſoul founds the thoughts and reflections which diſcover to it a great variety of intellectual truths. In order the better to comprehend how the ſenſes contribute to the advancement of knowledge, I begin with remarking, that the ſenſes act only on individual things, which actually exiſt under circumſtances determined or limited on all ſides.

Let us ſuppoſe a man ſuddenly placed in the world, poſſeſſed of all his faculties, but entirely deſtitute of experience; let a ſtone be put in his hand, let him then open that hand, and obſerve that the ſtone falls. This is an experiment limited on all ſides, which gives him no information, except that this ſtone being in the left hand, for example, and dropped, falls to the ground; he is by no means abſolutely certain that the ſame effect would enſue, were he to take another ſtone, or the ſame ſtone with his right hand.

It is ſtill uncertain whether this ſtone, under the ſame circumſtances, would again fall, or whether it would have fallen, had it been taken up an hour ſooner. This experiment alone gives him no light reſpecting theſe particulars.

The man in queſtion takes another ſtone, and obſerves that it falls likewiſe, whether dropped from the right hand or from the left; he repeats the experiment with a third and a fourth ſtone, and uniformly obſerves the ſame effect. He hence concludes that ſtones have the property of falling when dropped, or when that which ſupports them is withdrawn.

Here then is an article of knowledge which the man has derived from the experiments which he has made. He is very far from having made trial of every ſtone, or, ſuppoſing him to have done ſo, what certainty has he that the ſame thing would happen at all times? He knows nothing as to this, except what concerns the particular moments when he made the experiments; and what aſſurance has he that the ſame effect would take place in the hands of another man? Might he not think, that this quality of making ſtones fall was attached to his hands excluſively? A thouſand other doubts might ſtill be formed on the ſubject.

I have never, for example, made trial of the ſtones which compoſe the cathedral church of Magdeburg, and yet I have not the leaſt doubt, that all of them without exception are heavy, and that each of them would fall as ſoon as detached from the building.

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I even imagine that experience has ſupplied me with this knowledge, though I have never tried any one of thoſe ſtones.

This example is ſufficient to ſhew, how experiments made on individual objects only, have led mankind to the knowledge of univerſal propoſitions; but it muſt be admitted, that the underſtanding and the other faculties of the ſoul interfere in a manner which it would be extremely difficult clearly to unfold; and if we were determined to be over ſcrupulous about every circumſtance, no progreſs in ſcience could be made, for we ſhould be ſtopped ſhort at every ſtep.

It muſt be allowed, that the vulgar diſcover, in this reſpect, much more good ſenſe than thoſe ſcrupulous philoſophers, who are obſtinately determined to doubt of every thing. It is neceſſary, at the ſame time, to be on our guard againſt falling into the oppoſite extreme, by neglecting to employ the neceſſary precautions.

The three ſources from which our knowledge is derived, require all of them certain precautions, which muſt be carefully obſerved, in order to acquire aſſurance of the truth; but it is poſſible, in each, to carry matters too far, and it is always proper to ſteer a middle courſe.

The third ſource clearly proves this. It would undoubtedly be extreme folly to believe every thing [illegible] is told us; but exceſſive diſtruſt would be no [illegible]-worthy. He who is determined to doubt [illegible] thing, will never want a pretence: when a

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man ſays, or writes, that he has ſeen ſuch or ſuch an action, we may ſay at once, that is not true, and that the man takes amuſement in relating things which may excite ſurprize; and if his veracity is beyond ſuſpicion, it might be ſaid, that he did not ſee clearly, that his eyes were dazzled; and examples are to be found in abundance of perſons deceiving themſelves, falſely imagining they ſaw what they did not. The rules preſcribed, in this reſpect, loſe all their weight when you have to do with a wrangler.

Uſually, in order to be aſcertained of the truth of a recital or hiſtory, it is required, that the author ſhould have been himſelf a witneſs of what he relates, and that he ſhould have no intereſt in relating it differently from the truth. If afterwards two or more perſons relate the ſame thing, with the ſame circumſtances, it is juſtly conſidered as a ſtrong confirmation. Sometimes, however, a coincidence carried to extreme minuteneſs becomes ſuſpicious. For two perſons obſerving the ſame incident, ſee it in different points of view, and the one will always diſcern certain little circumſtances which the other muſt have overlooked. A ſlight difference in two ſeveral accounts of the ſame event, rather eſtabliſhes than invalidates the truth of it.

But it is always extremely difficult to reaſon on the firſt principles of our knowledge, and to attempt an explanation of the mechaniſm and of the moving powers which the ſoul employs. It would be glorious to ſucceed in ſuch an attempt, as it would elucidate a great variety of important points reſpecting the nature of the ſoul and its operations. But we ſeem deſtined rather to make uſe of our faculties, than to trace their nature through all its depths.

18th April, 1761.

LETTER VI.

Whether the Eſſence of Bodies be known by us.

AFTER ſo many reflections on the nature and faculties of the ſoul, you will not, perhaps, be diſpleaſed to return to the conſideration of body, the principal properties of which I have already endeavoured to explain.

I have remarked that the nature of body neceſſarily contains three things, *extenſion*, *impenetrability*, and *inertia*; ſo that a being, in which theſe three properties do not meet at once, cannot be admitted into the claſs of bodies; and reciprocally, when they are united in any one being, no one will heſitate to acknowledge it for a body.

In theſe three things, then, we are warranted to conſtitute the eſſence of body, though there are many philoſophers who pretend that the eſſence of bodies is wholly unknown to us. This is not only the opinion of the Pyrrhoniſts, who doubt of every thing; but there are other ſects likewiſe, who maintain, that the eſſence of all things is abſolutely unknown: and no doubt, in certain reſpects they have truth on their ſide: this is but too certain as to all the individual beings which exiſt.

You will eaſily comprehend, that it would be the height of abſurdity were I to pretend ſo much as to

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know the eſſence of the pen which I employ in writing this letter. If I knew the eſſence of this pen, (I ſpeak not of pens in general, but of that one only now between my fingers, which is an *individual being*, as it is called in metaphyſics, and which is diſtinguiſhed from all the other pens in the world;) if I knew, then, the eſſence of this individual pen, I ſhould be in a condition to diſtinguiſh it from every other, and it would be impoſſible to change it without my perceiving the change; I muſt know its nature thoroughly, the number and the arrangement of all the parts whereof it is compoſed. But how far am I from having ſuch a knowledge! Were I to riſe but for a moment, one of my children might eaſily change it, leaving another in its room, without my perceiving the difference; and were I even to put a mark upon it, how eaſily might that mark be counterfeited on another pen? And ſuppoſing this impoſſible for my children, it muſt always be admitted as poſſible for God to make another pen ſo ſimilar to this, that I ſhould be unable to diſcern any difference. It would be, however, another pen, really diſtinguiſhable from mine, and God would undoubtedly know the difference of them; in other words, God perfectly knows the eſſence of both the one and the other of theſe two pens: but as to me, who diſcern no difference, it is certain that the eſſence is altogether beyond my knowledge.

The ſame obſervation is applicable to all other individual things; and it may be confidently maintained, that God alone can know the eſſence or *nature of each. It* were impoſſible to fix on any one

LETTER VII.

The true Notion of Extenſion.

I HAVE already demonſtrated, that the general notion of body neceſſarily comprehends theſe three qualities, extenſion, impenetrability, and inertia, without which no being can be ranked in the claſs of bodies. Even the moſt ſcrupulous muſt allow the neceſſity of theſe three qualities, in order to conſtitute a body; but the doubt with ſome is, Are theſe three characters ſufficient? Perhaps, ſay they, there may be ſeveral other characters, which are equally neceſſary to the eſſence of body.

But I aſk: were God to create a being diveſted of theſe other unknown characters, and that it poſſeſſed only the three above mentioned, Would they heſitate to give the name of body to ſuch a being? No, aſſuredly; for if they had the leaſt doubt on the ſubject, they could not ſay with certainty, that the ſtones in the ſtreet are bodies, becauſe they are not ſure whether the pretended unknown characters are to be found in them or no.

Some imagine, that gravity is an eſſential property of all bodies, as all thoſe which we know are heavy; but were God to diveſt them of gravity, would they therefore ceaſe to be bodies? Let them conſider the heavenly bodies, which do not fall downward; as muſt be the caſe, if they were heavy as the bodies which we touch, yet they give them the ſame name.

And even on the ſuppoſition that all bodies were heavy, it would not follow that gravity is a property eſſential to them, for a body would ſtill remain a body, though its gravity were to be deſtroyed by a miracle.

But this reaſoning does not apply to the three eſſential properties above mentioned. Were God to annihilate the extenſion of a body, it would certainly be no longer a body; and a body diveſted of impenetrability would no longer be *body;* it would be a ſpectre, a phantom: the ſame holds as to inertia.

You know that extenſion is the proper object of geometry, which conſiders bodies only in ſo far as they are extended, abſtractedly from impenetrability and inertia; the object of geometry, therefore, is a notion much more general than that of body, as it comprehends not only bodies, but all beings ſimply extended without impenetrability, if any ſuch there be. Hence it follows, that all the properties deduced in geometry from the notion of extenſion muſt likewiſe take place in bodies, in as much as they are extended; for whatever is applicable to a more general notion, to that of a tree, for example, muſt likewiſe be applicable to the notion of an oak, an aſh, an elm, &c. and this principle is even the foundation of all the reaſonings in virtue of which we always affirm and deny of the ſpecies, and of individuals, every thing that we affirm and deny of the genus.

There are however philoſophers, particularly among our contemporaries, who boldly deny, that the properties applicable to extenſion, in general, that is, according as we conſider them in geometry, take place in bodies really exiſting. They allege that geometrical extenſion is an abſtract being, from the properties of which it is impoſſible to draw any concluſion, with reſpect to real objects: thus, when I have demonſtrated that the three angles of a triangle are together equal to two right angles, this is a property belonging only to an abſtract triangle, and not at all to one really exiſting.

But theſe philoſophers are not aware of the perplexing conſequences which naturally reſult from the difference which they eſtabliſh between objects formed by abſtraction, and real objects; and if it were not permitted to conclude from the firſt to the laſt, no concluſion, and no reaſoning whatever could ſubſiſt, as we always conclude from general notions to particular.

Now all general notions are as much abſtract beings as geometrical extenſion; and a tree, in general, or the general notion of trees, is formed only by abſtraction, and no more exiſts out of our mind than geometrical extenſion does. The notion of man in general is of the ſame kind, and man in general no where exiſts: all men who exiſt are individual beings, and correſpond to individual notions. The general idea which comprehends all, is formed only by abſtraction.

The fault which theſe philoſophers are ever finding with geometricians, for employing themſelves about abſtractions merely, is therefore groundleſs, as all other ſciences principally turn on general notions, which are no more real than the objects of geometry. The patient, in general, whom the phyſician has in view, and the idea of whom contains all patients really exiſting, is only an abſtract idea; nay the very merit of each ſcience is ſo much the greater, as it extends to notions more general, that is to ſay, more abſtract.

I ſhall endeavour, by next poſt, to point out the tendency of the cenſures pronounced by theſe philoſophers upon geometricians; and the reaſons why they are unwilling that we ſhould aſcribe to real, extended beings, that is, to exiſting bodies, the properties applicable to extenſion in general; or to abſtracted extenſion. They are afraid leſt their metaphyſical principles ſhould ſuffer in the cauſe.

25th April, 1761.

LETTER VIII.

Diviſibility of Extenſion in infinitum.

THE controverſy between modern philoſophers and geometricians to which I have alluded, turns on the diviſibility of body. This property is undoubtedly founded on extenſion, and it is only in ſo far as bodies are extended that they are diviſible, and capable of being reduced to parts.

You will recollect that in geometry it is always poſſible to divide a line, however ſmall, into two equal parts. We are likewiſe, by that ſcience, into admit of no farther diviſion; each part, then, would no longer have any length, for if it had any, it would be ſtill diviſible. Each particle, then, would of conſequence be a nothing. But if theſe thouſand particles together conſtituted the length of an inch, the thouſandth part of an inch would, of conſequence, be a nothing; which is equally abſurd with maintaining, that the half of any quantity whatever is nothing. And if it be abſurd to affirm, that the half of any quantity is nothing, it is equally ſo to affirm, that the half of a half, or that the fourth part of the ſame quantity, is nothing; and what muſt be granted as to the fourth, muſt likewiſe be granted with reſpect to the thouſandth, and the millionth part. Finally, however far you may have already carried, in imagination, the diviſion of an inch, it is always poſſible to carry it ſtill farther; and never will you be able to carry on your ſubdiviſion ſo far, as that the laſt parts ſhall be abſolutely indiviſible. Theſe parts will undoubtedly always become ſmaller, and their magnitude will approach nearer and nearer to 0, but can never reach it.

The geometrician, therefore, is warranted in affirming, that every magnitude is diviſible to infinity; and that you cannot proceed ſo far in your diviſion, as that all farther diviſion ſhall be impoſſible. But it is always neceſſary to diſtinguiſh between what is poſſible in itſelf, and what we are in a condition to perform. Our execution is indeed extremely limited. example, divided an inch into a thouſand parts, theſe parts are ſo ſmall as to eſcape our ſenſes, and a farther diviſion would to us, no doubt, be impoſſible.

But you have only to look at this thouſandth part of an inch through a good microſcope, which magnifies, for example, a thouſand times, and each particle will appear as large as an inch to the naked eye; and you will be convinced of the poſſibility of dividing each of theſe particles again into a thouſand parts: the ſame reaſoning may always be carried forward, without limit and without end.

It is therefore an indubitable truth, that all magnitude is diviſible in infinitum, and that this takes place not only with reſpect to extenſion, which is the object of geometry, but likewiſe with reſpect to every other ſpecies of quantity, ſuch as time and number.

28th April, 1761.

LETTER IX.

Whether this Diviſibility in infinitum *takes place in exiſting Bodies?*

IT is, then, a completely eſtabliſhed truth, that extenſion is diviſible to infinity, and that it is impoſſible to conceive parts ſo ſmall as to be unſuſceptible of farther diviſion. Philoſophers accordingly do not impugn this truth itſelf, but deny that it takes place in exiſting bodies. They allege, that extenſion, the diviſibility of which to infinity has been demonſtrated, is merely a chimerical object, formed by abſtraction; and that ſimple extenſion, as conſidered in geometry, can have no real exiſtence.

Here they are in the right; and extenſion is undoubtedly a general idea, formed in the ſame manner as that of man, or of tree in general, by abſtraction; and as man or tree in general exiſts not, no more does extenſion in general exiſt. You are perfectly ſenſible, that individual beings alone exiſt, and that general notions are to be found only in the mind; but it cannot therefore be maintained that theſe general notions are chimerical; they contain, on the contrary, the foundation of all our knowledge.

Whatever applies to a general notion, and all the properties attached to it, of neceſſity take place in all the individuals comprehended under that general notion. When it is affirmed that the general notion of man contains an underſtanding and a will, it is undoubtedly meant, that every individual man is endowed with thoſe faculties. And how many properties do theſe very philoſophers boaſt of having demonſtrated as belonging to ſubſtance in general, which is ſurely an idea as abſtract as that of extenſion; and yet they maintain, that all theſe properties apply to all individual ſubſtances, which are all extended. If, in effect, ſuch a ſubſtance had not theſe properties, it would be falſe that they belonged to ſubſtance in general.

If bodies, which infallibly are extended because endowed with extenſion, were not diviſible to infinity, it would be likewiſe falſe, that diviſibility in infinitum is a property of extenſion. Now thoſe philoſophers readily admit that this property belongs to extenſion, but they inſiſt that it cannot take place in extended beings. This is the ſame thing with affirming, that the underſtanding and will are indeed attributes of the notion of man in general; but that they can have no place in individual men actually exiſting.

Hence you will readily draw this concluſion: if diviſibility in infinitum is a property of extenſion in general, it muſt of neceſſity likewiſe belong to all individual extended beings; or if real extended beings are not diviſible to infinity, it is falſe that diviſibility in infinitum can be a property of extenſion in general.

It is impoſſible to deny the one or the other of theſe conſequences without ſubverting the moſt ſolid principles of all knowledge; and the philoſophers who refuſe to admit diviſibility in infinitum in real extended beings, ought as little to admit it with reſpect to extenſion in general; but as they grant this laſt, they fall into a glaring contradiction.

You need not to be ſurprized at this; it is a failing from which the greateſt men are not exempt. But what is rather ſurprizing, theſe philoſophers, in order to get rid of their embarraſſment, have thought proper to deny that body is extended. They ſay, that it is only an appearance of extenſion which is perceived in bodies, but that real extenſion by no means belongs to them.

You ſee clearly that this is merely a wretched cavil, by which the principal, and the moſt evident property of body is denied. It is an extravagance ſimilar to that formerly imputed to the Epicurean philoſophers, who maintained that every thing which exiſts in the univerſe is material, without even excepting the gods whoſe exiſtence they admitted. But as they ſaw that theſe corporeal gods would be ſubjected to the greateſt difficulties, they invented a ſubterfuge ſimilar to that of our modern philoſophers, alleging, That the gods had not bodies, but *as it were* bodies, (*quaſi corpora*,) and that they had not ſenſes, but ſenſes *as it were*; and ſo of all the members. The other philoſophical ſects of antiquity made themſelves abundantly merry with theſe *quaſi-corpora* and *quaſi-ſenſus*; and they would have equal reaſon, in modern times, to laugh at the *quaſi-extenſion* which our philoſophers aſcribe to body; this term *quaſi-extenſion* ſeems perfectly well to expreſs that appearance of extenſion, without being ſo in reality.

Geometricians, if they meant to confound them, have only to ſay, that the objects whoſe diviſibility in infinitum they have demonſtrated, were likewiſe only *as it were* extended, and that accordingly all bodies extended *as it were*, were neceſſarily diviſible in infinitum. But nothing is to be gained with them; they are reſolute to maintain the greateſt abſurdities rather than acknowledge a miſtake. You muſt have remarked, that this is the character of almoſt all ſcholars.

3d May, 1761.

LETTER

LETTER X.

Of Monads.

WHEN we talk, in company, on philoſophical ſubjects, the converſation uſually turns on ſuch articles as have excited violent diſputes among philoſophers.

The diviſibility of body is one of them, reſpecting which the ſentiments of the learned are greatly divided. Some maintain, that this diviſibility goes on to infinity, without the poſſibility of ever arriving at particles ſo ſmall as to be ſuſceptible of no farther diviſion. But others inſiſt, that this diviſion extends only to a certain point, and that you may come at length to particles ſo minute, that, having no magnitude, they are no longer diviſible. Theſe ultimate particles, which enter into the compoſition of bodies, they denominate *ſimple beings*, and *monads*.

There was a time when the diſpute reſpecting monads employed ſuch general attention, and was conducted with ſo much warmth, that it forced its way into company of every deſcription, that of the guard-room not excepted. There was ſcarce a lady at court who did not take a decided part in favour of monads or againſt them. In a word, all converſation was engroſſed by monads, no other ſubject could find admiſſion.

The Royal Academy of Berlin took up the controverſy, and being accuſtomed annually to propoſe a queſtion for diſcuſſion, and to beſtow a gold medal of the value of fifty ducats on the perſon who in the judgment of the Academy has given the moſt ingenious ſolution, the queſtion reſpecting monads was ſelected for the year 1748. A great variety of eſſays on the ſubject were accordingly produced. The preſident, Mr. de Maupertuis, named a committee to examine them, under the direction of the late Count Dohna, great chamberlain to the queen; who, being an impartial judge, examined, with all imaginable attention, the arguments adduced both for and againſt the exiſtence of monads. Upon the whole, it was found that thoſe which went to the eſtabliſhment of their exiſtence were ſo feeble, and ſo chimerical, that they tended to the ſubverſion of all the principles of human knowledge. The queſtion was, therefore, determined in favour of the oppoſite opinion, and the prize adjudged to Mr. Juſti, whoſe piece was deemed the moſt complete refutation of the monadiſts.

You may eaſily imagine how violently this deciſion of the Academy muſt irritate the partiſans of monads, at the head of whom ſtood the celebrated Mr. Wolff. His followers, who were then much more numerous, and more formidable than at preſent, exclaimed in high terms againſt the partiality and injuſtice of the Academy; and their chief had well nigh proceeded to launch the thunder of a philoſophical anathema againſt it. I do not now recollect to whom we are indebted for the care of averting this diſaſter.

As this controverſy has made a great deal of noiſe, you will not be diſpleaſed, undoubtedly, if I dwell a little upon it. The whole is reduced to this ſimple queſtion, Is body diviſible to infinity? or, in other words, Has the diviſibility of bodies any bound, or has it not? I have already remarked as to this, that extenſion, geometrically conſidered, is on all hands allowed to be diviſible in infinitum; becauſe, however ſmall a magnitude may be, it is poſſible to conceive the half of it, and again the half of that half, and ſo on to infinity.

This notion of extenſion is very abſtract, as are thoſe of all *genera*, ſuch as that of man, of horſe, of tree, &c. as far as they are not applied to an individual and determinate being. Again, it is the moſt certain principle of all our knowledge, that whatever can be truly affirmed of the genus, muſt be true of all the individuals comprehended under it. If therefore all bodies are extended, all the properties belonging to extenſion muſt belong to each body in particular. Now all bodies are extended; and extenſion is diviſible to infinity; therefore every body muſt be ſo likewiſe. This is a ſyllogiſm of the beſt form; and as the firſt propoſition is indubitable, all that remains, is to be aſſured that the ſecond is true, that is, whether it be true or not, that bodies are extended.

The partiſans of monads, in maintaining their opinion, are obliged to affirm, that bodies are not extended, but have only an appearance of extenſion. They imagine that by this they have ſubverted the argument adduced in ſupport of the diviſibility in infinitum. But if body is not extended, I ſhould be glad to know, from whence we derived the idea of extenſion; for, if body is not extended, nothing in the world is, as ſpirits are ſtill leſs ſo. Our idea of extenſion, therefore, would be altogether imaginary and chimerical.

Geometry would accordingly be a ſpeculation entirely uſeleſs and illuſory, and never could admit of any application to things really exiſting. In effect, if no one thing is extended, to what purpoſe inveſtigate the properties of extenſion? But as geometry is, beyond contradiction, one of the moſt uſeful of ſciences, its object cannot poſſibly be a mere chimera.

There is a neceſſity, then, of admitting, that the object of geometry is at leaſt the ſame apparent extenſion which thoſe philoſophers allow to body; but this very object is diviſible to infinity: therefore exiſting beings, endowed with this apparent extenſion, muſt neceſſarily be extended.

Finally, let thoſe philoſophers turn themſelves which way ſoever they will in ſupport of their monads, or thoſe ultimate and minute particles, diveſted of all magnitude, of which, according to them, all bodies are compoſed, they ſtill plunge into difficulties, out of which they cannot extricate themſelves. They are right in ſaying, that it is a proof of dulneſs to be incapable of reliſhing their ſublime doctrine; it may however be remarked, that here the greateſt ſtupidity is the moſt ſucceſsful.

LETTER XI.

Reflections on Diviſibility in infinitum, *and on Monads.*

IN ſpeaking of the diviſibility of body, we muſt carefully diſtinguiſh what is in our power, from what is poſſible in itſelf. In the firſt ſenſe, it cannot be denied, that ſuch a diviſion of body as we are capable of, muſt be very limited.

By pounding a ſtone we can eaſily reduce it to powder; and if it were poſſible to reckon all the little grains which form that powder, their number would undoubtedly be ſo great, that it would be matter of ſurprize, to have divided the ſtone into ſo many parts. But theſe very grains will be almoſt indiviſible with reſpect to us, as no inſtrument we could employ will be able to lay hold of them. But it cannot with truth be affirmed that they are indiviſible in themſelves. You have only to view them with a good microſcope, and each will appear itſelf a conſiderable ſtone, on which are diſtinguiſhable a great many points and inequalities; which demonſtrates the poſſibility of a farther diviſion, though we are not in a condition to execute it. For whereever we can diſtinguiſh ſeveral points in any object, it muſt be diviſible into ſo many parts.

We ſpeak not, therefore, of a diviſion practicable by our ſtrength and ſkill, but of that which is poſſible in itſelf, and which the Divine Omnipotence is able to accompliſh.

It is in this ſenſe, accordingly, that philoſophers uſe the word 'diviſibility:' ſo that if there were a ſtone ſo hard that no force could break it, it might be without heſitation affirmed as diviſible in its own nature, as the moſt brittle, of the ſame magnitude. And how many bodies are there on which we cannot lay any hold, and of whoſe diviſibility we can entertain not the ſmalleſt doubt? No one doubts that the moon is a diviſible body, though he is incapable of detaching the ſmalleſt particle from it: and the ſimple reaſon for its diviſibility, is its being extended.

Wherever we remark extenſion, we are under the neceſſity of acknowledging diviſibility, ſo that diviſibility is an inſeparable property of extenſion. But experience likewiſe demonſtrates that the diviſion of bodies extends very far. I ſhall not inſiſt at great length on the inſtance uſually produced of a ducat: the artiſan can beat it out into a leaf ſo fine, as to cover a very large ſurface, and the ducat may be divided into as many parts as that ſurface is capable of being divided. Our own body furniſhes an example much more ſurprizing. Only conſider the delicate veins and nerves with which it is filled, and the fluids which circulate through them. The ſubtilty there diſcoverable far ſurpaſſes imagination.

The ſmalleſt inſects, ſuch as are ſcarcely viſible to the naked eye, have all their members, and legs on which they walk with amazing velocity. Hence we ſee that each limb has its muſcles compoſed of a great number of fibres; that they have veins, and nerves, and a fluid ſtill much more ſubtile which flows through their whole extent.

On viewing with a good microſcope a ſingle drop of water, it has the appearance of a ſea; we ſee thouſands of living creatures ſwimming in it, each of which is neceſſarily compoſed of an infinite number of muſcular and nervous fibres, whoſe marvellous ſtructure ought to excite our admiration. And though theſe creatures may perhaps be the ſmalleſt which we are capable of diſcovering by the help of the microſcope, undoubtedly they are not the ſmalleſt which the Creator has produced. Animalcules probably exiſt as ſmall relatively to them, as they are relatively to us. And theſe after all are not yet the ſmalleſt, but may be followed by an infinity of new claſſes, each of which contains creatures incomparably ſmaller than thoſe of the preceding claſs.

We ought in this to acknowledge the omnipotence and infinite wiſdom of the Creator, as in objects of the greateſt magnitude. It appears to me, that the conſideration of theſe minute ſpecies, each of which is followed by another inconceivably more minute, ought to make the livelieſt impreſſion on our minds, and inſpire us with the moſt ſublime ideas of the works of the Almighty, whoſe power knows no bounds, whether as to great objects or ſmall.

To imagine that after having divided a body into a great number of parts, we arrive, at length, at particles ſo ſmall as to defy all farther diviſion, is therefore the indication of a very contracted mind. But ſuppoſing it poſſible to deſcend to particles ſo minute as to be, in their own nature, no longer diviſible, as in the caſe of the ſuppoſed monads; before coming to this point, we ſhall have a particle compoſed of only two monads, and this particle will be of a certain magnitude or extenſion, otherwiſe it could not have been diviſible into theſe two monads. Let us farther ſuppoſe, that this particle, as it has ſome extenſion, may be the thouſandth part of an inch, or ſtill ſmaller if you will; for it is of no importance, what I ſay of the thouſandth part of an inch may be ſaid with equal truth of every ſmaller part. This thouſandth part of an inch, then, is compoſed of two monads, and conſequently two monads together would be the thouſandth part of an inch, and two thouſand times nothing, a whole inch; the abſurdity ſtrikes at firſt ſight.

The partiſans of the ſyſtem of monads accordingly ſhrink from the force of this argument, and are reduced to a terrible nonplus when aſked how many monads are requiſite to conſtitute an extenſion. Two, they apprehend, would appear inſufficient, they therefore allow that more muſt be neceſſary. But, if two monads cannot conſtitute extenſion, as each of the two has none; neither three, nor four, nor any number whatever will produce it; and this completely ſubverts the ſyſtem of monads.

9th May, 1761.

LETTER XII.

Reply to the Objections of the Monadiſts to Diviſibility in infinitum.

THE partiſans of monads are far from ſubmitting to the arguments adduced to eſtabliſh the diviſibility of body to infinity. Without attacking them directly, they allege that diviſibility in infinitum is a chimera of geometricians, and that it is involved in contradiction. For, if each body is diviſible to infinity, it would contain an infinite number of parts, the ſmalleſt bodies as well as the greateſt: the number of theſe particles to which diviſibility in infinitum would lead, that is to ſay, the moſt minute of which bodies are compoſed, will then be as great in the ſmalleſt body as in the largeſt, this number being *infinite* in both; and hence the partiſans of monads triumph in their reaſoning as invincible. For, if the number of ultimate particles of which two bodies are compoſed is the ſame in both, it muſt follow, ſay they, that the bodies are perfectly equal to each other.

Now this goes on the ſuppoſition, that the ultimate particles are all perfectly equal to each other; for if ſome were greater than others, it would not be ſurprizing that one of the two bodies ſhould be much greater than the other. But it is abſolutely neceſſary, ſay they, that the ultimate particles of all bodies ſhould be equal to each other, as they no longer have any extenſion, and their magnitude abſolutely vaniſhes,

niſhes, or becomes nothing. They even form a new objection, by alleging that all bodies would be compoſed of an infinite number of nothings, which is a ſtill greater abſurdity.

I readily admit this; but I remark at the ſame time, that it ill becomes them to raiſe ſuch an objection, ſeeing they maintain, that all bodies are compoſed of a certain number of monads, though, relatively to magnitude, they are abſolute nothings: ſo that by their own confeſſion, ſeveral nothings are capable of producing a body. They are right in ſaying their monads are not nothings, but beings endowed with an excellent quality, on which the nature of the bodies which they compoſe is founded. Now, the only queſtion here is reſpecting extenſion; and as they are under the neceſſity of admitting that their monads have none, ſeveral nothings, according to them, would always be ſomething.

But I ſhall puſh this argument againſt the ſyſtem of monads no farther; my object being to make a direct reply to the objection founded on the ultimate particles of bodies, raiſed by the monadiſts in ſupport of their ſyſtem, by which they flatter themſelves in the confidence of a complete victory over the partiſans of diviſibility in infinitum.

I ſhould be glad to know, in the firſt place, what they mean by the *ultimate particles* of bodies. In their ſyſtem, according to which every body is compoſed of a certain number of monads, I clearly comprehend that the ultimate particles of a body, are the monads themſelves which conſtitute it; but in the ſyſtem of diviſibility in infinitum, the term ultimate particle is abſolutely unintelligible.

They are right in ſaying, that theſe are the particles at which we arrive from the diviſion of bodies, after having continued it to infinity. But this is juſt the ſame thing with ſaying, after having finiſhed a diviſion which never comes to an end. For diviſibility in infinitum means nothing elſe but the poſſibility of always carrying on the diviſion, without ever arriving at the point where it would be neceſſary to ſtop. He who maintains diviſibility in infinitum, boldly denies, therefore, the exiſtence of the ultimate particles of body; and it is a manifeſt contradiction, to ſuppoſe at once ultimate particles and diviſibility in infinitum.

I reply, then, to the partiſans of the ſyſtem of monads, that their objection to the diviſibility of body to infinity would be a very ſolid one, did that ſyſtem admit of ultimate particles; but being expreſsly excluded from it, all this reaſoning, of courſe, falls to the ground.

It is falſe, therefore, that in the ſyſtem of diviſibility in infinitum, bodies are compoſed of an infinity of particles. However cloſely connected theſe two propoſitions may appear to the partiſans of monads, they manifeſtly contradict each other; for whoever maintains that body is diviſible in infinitum, or without end, abſolutely denies the exiſtence of ultimate particles, and conſequently has no concern in the queſtion. The term can only mean ſuch particles as are no longer diviſible, an idea totally inconſiſtent with the ſyſtem of diviſibility in infinitum. This formidable attack, then, is completely repelled.

12th May, 1761.

LETTER XIII.

Principle of the ſatisfying Reaſon, *the ſtrongeſt Support of the Monadiſts.*

YOU muſt be perfectly ſenſible that one of the two ſyſtems, which have undergone ſuch ample diſcuſſion, is neceſſarily true, and the other falſe, ſeeing they are contradictory.

It is admitted on both ſides, that bodies are diviſible: the only queſtion is, Whether this diviſibility is limited? or Whether it may always be carried farther, without the poſſibility of ever arriving at indiviſible particles?

The ſyſtem of monads is eſtabliſhed in the former caſe, ſince after having divided a body into indiviſible particles, theſe very particles are monads, and there would be reaſon for ſaying that all bodies are compoſed of them, and each of a certain determinate number. Whoever denies the ſyſtem of monads, muſt likewiſe, then, deny that the diviſibility of bodies is limited. He is under the neceſſity of maintaining, that it is always poſſible to carry this diviſibility farther, without ever being obliged to ſtop; and this is the caſe of diviſibility in infinitum, on which ſyſtem we abſolutely deny the exiſtence of ultimate particles: conſequently the difficulties reſul ing from their infinite number fall to the ground of themſelves. In denying monads, it is impoſſible to talk any longer of ultimate particles, and ſtill leſs of the number of them which enters into the compoſition of each body.

You muſt have remarked, that what I have hitherto produced in ſupport of the ſyſtem of monads is deſtitute of ſolidity. I now proceed to inform you that its ſupporters reſt their cauſe chiefly on the great principle of the *ſufficient reaſon*, which they know how to employ ſo dexterouſly, that by means of it they are in a condition to demonſtrate whatever ſuits their purpoſe, and to demoliſh whatever makes againſt them. The bleſſed diſcovery made, then, is this, *That nothing can be without a ſufficient reaſon;* and to modern philoſophers we ſtand indebted for it.

In order to give you an idea of this principle, you have only to conſider, that in every thing preſented to you, it may always be aſked, Why it is ſuch? And the anſwer is what they call the *ſufficient reaſon*, ſuppoſing it really to correſpond with the queſtion propoſed. Wherever the *why* can take place, the poſſibility of a ſatisfactory anſwer is taken for granted, which ſhall, of courſe, contain the ſufficient reaſon of the thing.

This is very far, however, from being a myſtery of modern diſcovery. Men in every age have aſked *why*; an inconteſtable proof of their conviction that every thing muſt have a ſatisfying reaſon of its exiſtence. This principle, that *nothing is without a cauſe*, well known to ancient philoſophers; but unhappily this cauſe is for the moſt part concealed from us. To little purpoſe do we aſk *why:* no one is qualified to aſſign the reaſon. It is not a matter of doubt, that every thing has its cauſe; but a progreſs thus far hardly deſerves the name; and ſo long as it remains concealed, we have not advanced a ſingle ſtep in real knowledge.

You may perhaps imagine, that modern philoſophers, who make ſuch a boaſt of the principle of a ſatisfying reaſon, have actually diſcovered that of all things, and are in a condition to anſwer every *why* that can be propoſed to them; which would undoubtedly be the very ſummit of human knowledge; but, in this reſpect, they are juſt as ignorant as their neighbours: their whole merit amounts to no more than a pretenſion to have demonſtrated, that whereever it is poſſible to aſk the queſtion *why*, there muſt be a ſatisfying anſwer to it, though concealed from us.

They readily admit, that the ancients had a knowledge of this principle, but a knowledge very obſcure; whereas they pretend to have placed it in its cleareſt light, and to have demonſtrated the truth of it: and therefore it is that they know how to turn it moſt to their account, and that this principle puts them in a condition to prove, that bodies are compoſed of monads.

Bodies, ſay they, muſt have their ſufficient reaſon ſomewhere; but if they were diviſible to infinity, ſuch reaſon could not take place: and hence they conclude, with an air altogether philoſophic, *that, as every thing muſt have its ſufficient reaſon, it is abſolutely* *neceſſary that all bodies ſhould be compoſed of monads:* which was to be demonſtrated. This, I muſt admit, is a demonſtration not to be reſiſted.

It were greatly to be wiſhed that a reaſoning ſo ſlight could elucidate to us queſtions of this importance; but I frankly confeſs, I comprehend nothing of the matter. They talk of the ſufficient reaſon of bodies, by which they mean to reply to a certain *wherefore*, which remains unexplained. But it would be proper, undoubtedly, clearly to underſtand, and carefully to examine a queſtion, before a reply is attempted; in the preſent caſe, the anſwer is given before the queſtion is formed.

Is it aſked, Why do bodies exiſt? It would be ridiculous, in my opinion, to reply, Becauſe they are compoſed of monads; as if they contained the cauſe of that exiſtence. Monads have not created bodies: and when I aſk, Why ſuch a being exiſts? I ſee no other reaſon that can be given but this, Becauſe the Creator has given it exiſtence; and as to the manner in which creation is performed, philoſophers, I think, would do well honeſtly to acknowledge their ignorance.

But they maintain, that God could not have produced bodies, without having created monads, which were neceſſary to form the compoſition of them. This manifeſtly ſuppoſes, that bodies are compoſed of monads, the point which they meant to prove by this reaſoning. And you are abundantly ſenſible, that it is not fair reaſoning to take for granted the truth of a propoſition which you are bound to prove by reaſoning. It is a ſophiſm known in logic by the name of a *petitio principii*, or, begging the queſtion.

16th May, 1761.

LETTER XIV.

Another Argument of the Monadiſts, derived from the Principle of the ſufficient Reaſon. *Abſurdities reſulting from it.*

THE partiſans of monads likewiſe derive their grand argument from the principle of the ſatisfying reaſon, by alleging that they could not even comprehend the poſſibility of bodies, if they were diviſible to infinity, as there would be nothing in them capable of checking imagination: they muſt have ultimate particles or elements, the compoſition of which muſt ſerve to explain the compoſition of bodies.

But do they pretend to underſtand the poſſibility of all the things which exiſt? This would ſavour too much of pride; nothing is more common among philoſophers than this kind of reaſoning: I cannot comprehend the poſſibility of this, unleſs it is ſuch as I imagine it to be: therefore it neceſſarily muſt be ſuch.

You clearly comprehend the frivolouſneſs of ſuch reaſoning; and that in order to arrive at truth, reſearch much more profound muſt be employed. Ignorance can never become an argument to conduct us to the knowledge of truth, and the one in queſtion is evidently founded on ignorance of the different manners which may render the thing poſſible.

But on the ſuppoſition that nothing exiſts but that whoſe poſſibility they are able to comprehend, is it poſſible for them to explain how bodies would be compoſed of monads? Monads, having no extenſion, muſt be conſidered as points in geometry, or as we repreſent to ourſelves ſpirits and ſouls. Now it is well known that many geometrical points, let the number be ſuppoſed ever ſo great, never can produce a line, and conſequently ſtill leſs a ſurface, or a body. If a thouſand points were ſufficient to conſtitute the thouſandth part of an inch, each of theſe muſt neceſſarily have an extenſion, which, taken a thouſand times, would become equal to the thouſandth part of an inch. Finally, it is an inconteſtable truth, that take any number of points you will, they never can produce extenſion. I ſpeak here of points ſuch as we conceive in geometry, without any length, breadth or thickneſs, and which in that reſpect, are abſolutely nothing.

Our philoſophers accordingly admit that no extenſion can be produced by geometrical points, and they ſolemnly proteſt that their monads ought not to be confounded with theſe points. They have no more extenſion than points, ſay they; but they are inveſted with admirable qualities, ſuch as repreſenting to them the whole univerſe by ideas, though extremely obſcure; and theſe qualities render them proper to produce the phenomenon of extenſion, or rather that apparent extenſion which I formerly mentioned. The ſame idea, then, ought to be formed of monads as of ſpirits and ſouls, with this difference, that the faculties of monads are much more imperfect.

The difficulty appears to me by this greatly increaſed, and I flatter myſelf you will be of my opinion, that two or more ſpirits cannot poſſibly be joined ſo as to form extenſion. Several ſpirits may very well form an aſſembly, or a council, but never an extenſion; abſtraction made of the body of each counſellor, which contributes nothing to the deliberation going forward, for this is the production of ſpirits only; a council is nothing elſe but an aſſembly of ſpirits or ſouls: but could ſuch an aſſembly repreſent an extenſion? Hence it follows, that monads are ſtill leſs proper to produce extenſion than geometrical points are.

The partiſans of the ſyſtem, accordingly, are not agreed as to this point. Some allege, that monads are actual parts of bodies; and that after having divided a body as far as poſſible, you then arrive at the monads which conſtitute it.

Others abſolutely deny that monads can be conſidered as conſtituent parts of bodies; according to them, they contain only the ſufficient reaſon: while the body is in motion, the monads ſtir not, but they contain the ſufficient reaſon of motion. Finally, they cannot touch each other; thus, when my hand touches a body, no one monad of my hand touches a monad of the body.

What is it then, you will aſk, that touches in this caſe, if it is not the monads which compoſe the hand and the body? The anſwer muſt be, that two nothings touch each other, or rather it muſt be denied that there is a real contact. It is a mere illuſion deſtitute of all foundation. They are under the neceſſity of affirming the ſame thing of all bodies, which according to theſe philoſophers are only phantoms formed by the imagination, repreſenting to itſelf very confuſedly the monads which contain the ſufficient reaſon of all that we denominate body.

In this philoſophy every thing is ſpirit, phantom and illuſion; and when we cannot comprehend theſe myſteries, it is our ſtupidity that keeps up an attachment to the groſs notions of the vulgar.

The greateſt ſingularity in the caſe is, that theſe philoſophers, with a deſign to inveſtigate and explain the nature of bodies and of extenſion, are at laſt reduced to deny their exiſtence. This is undoubtedly the ſureſt way to ſucceed in explaining the phenomena of nature; you have only to deny them, and to allege, in proof, the principle of the ſufficient reaſon. Into ſuch extravagancies will philoſophers run, rather than acknowledge their ignorance.

19th May, 1761.

LETTER XV.

Reflections on the Syſtem of Monads.

IT would be a great pity, however, that this ingenious ſyſtem of monads ſhould crumble into ruins. It has made too much noiſe, it has coſt its partiſans too many ſublime and profound ſpeculations, to be permitted to ſink into total oblivion. It will ever remain a ſtriking monument of the extravagance into which the ſpirit of philoſophizing may run. It is well worth while, then, to preſent you with a more particular account of it.

It is neceſſary, firſt of all, to baniſh from the mind every thing corporeal, all extenſion, all motion, all time and ſpace, for all theſe are mere illuſion. Nothing exiſts in the world but monads, the number of which undoubtedly is prodigious. No one monad is to be found in connection with others; and it is demonſtrated by the principle of the ſufficient reaſon, that monads can in no manner whatever act upon each other. They are indeed inveſted with powers, but theſe exerted only within themſelves, without having the leaſt influence externally.

Theſe powers, with which each monad is endowed, have a tendency only to be continually changing their own ſtate, and conſiſt in the repreſentation of all other monads. My ſoul, for example, is a monad, and contains in itſelf ideas of the ſtate of all other monads. Theſe ideas are for the moſt part very obſcure; but the powers of my ſoul are continually employed in their farther elucidation, and in carrying them to a higher degree of clearneſs. Other monads have, in this reſpect, a ſufficient reſemblance to my ſoul; each is replete with a prodigious quantity of obſcure ideas of all other monads, and of their ſtate; and they are continually exerting themſelves with more or leſs ſucceſs in unfolding theſe ideas, and in carrying them to a higher degree of clearneſs.

Such monads as have ſucceeded better than I have done are ſpirits more perfect; but the greater part ſtill remain in a ſtate of ſtagnation, in the greateſt obſcurity of their ideas; and when they are the object of the ideas of my ſoul, they produce in it the illuſory and chimerical idea of extenſion, and of body. As often as my ſoul thinks of bodies and of motion, this proves that a great quantity of other monads are ſtill buried in their obſcurity; it is likewiſe when I think of them, that my ſoul forms within itſelf the idea of ſome extenſion, which is conſequently nothing but mere illuſion.

The more monads there are plunged in the abyſs of the obſcurity of their ideas, the more is my ſoul dazzled with the idea of extenſion; but when they come to clear up their obſcure ideas, extenſion ſeems to me to diminiſh, and this produces in my ſoul the illuſory idea of motion.

You will aſk, no doubt, How my ſoul perceives that other monads ſucceed in developing their obſcure ideas, ſeeing there is no connection between them and me? The partiſans of the ſyſtem of monads are ready with this reply, that it takes place conformably to the perfect harmony which the Creator (who is himſelf only a monad,) has eſtabliſhed between monads, by which each perceives in itſelf, as in a mirror, every developement produced in others, without any manner of connection between them.

It is to be hoped, then, that all monads may at length become ſo happy as to clear up their obſcure ideas, and then we ſhould loſe all ideas of body and of motion; and the illuſion, ariſing merely from the obſcurity of ideas, would entirely ceaſe.

But there is little appearance of the arrival of this bleſſed ſtate; moſt monads, after having acquired the capacity of clearing up their obſcure ideas, ſuddenly relapſe. When ſhut up in my chamber, I perceive myſelf but of ſmall extenſion, becauſe ſeveral monads have then unfolded their ideas: but as ſoon as I walk abroad, and contemplate the vaſt expanſe of heaven, they muſt all have relapſed into their ſtate of dulneſs.

There is no change of place or of motion; all that is illuſion merely: my ſoul remains almoſt always in the ſame place, juſt as all other monads. But, when it begins to unfold ſome ideas, which before were but very obſcure, it appears to me then that I am approaching the object which they repreſent to me, or rather that which the monads of ſuch idea excite in me: and this is the real explanation of the phenomenon, when it appears to us that we are approaching to certain objects.

It happens but too frequently that the elucidations we had acquired are again loſt; then it appears to us that we are removing from the ſame object. And here we muſt look for the true ſolution of our journeyings. My idea, for example, of the city of Magdeburg is produced by certain monads, of which at preſent I have but very obſcure ideas; and this is the reaſon why I conſider myſelf as at a diſtance from Magdeburg. Laſt year, theſe ſame ideas ſuddenly became clear, and then I imagined I was travelling to Magdeburg, and that I remained there ſeveral days. This journey, however, was an illuſion merely, for my ſoul never ſtirs from its place. It is likewiſe an illuſion when you imagine yourſelf abſent from Berlin, becauſe the confuſed repreſentation of certain monads excites an obſcure idea of Berlin, which you have only to clear up, and that inſtant you are at Berlin. Nothing more is neceſſary. What we call journeys, and on which we expend ſo much money, is mere illuſion. Such is the real plan of the ſyſtem of monads.

You will aſk, Is it poſſible there ever ſhould have been perſons of good ſenſe, who ſeriouſly maintained theſe extravagances? I reply, there have been but too many, that I know ſeveral of them, that there are ſome at Berlin, nay perhaps at Magdeburg.

23d May, 1761.

LETTER XVI.

Continuation.

THE ſyſtem of monads, ſuch as I have been deſcribing it, is a neceſſary conſequence from the principle, that bodies are compounded of ſimple beings. The moment this principle is admitted, you are obliged to acknowledge the juſtneſs of all the other conſequences, which reſult from it ſo naturally, that it is impoſſible to reject any one, however abſurd and contradictory.

Firſt, theſe ſimple beings, which muſt enter into the compoſition of bodies, being monads which have no extenſion, neither can their compounds, that is bodies, have any; and all theſe extenſions become illuſion, chimera, it being certain, that parts deſtitute of extenſion are incapable of producing a real extenſion; it can be, at moſt, an appearance, or a phantom which dazzles by a fallacious idea of extenſion. In a word, every thing becomes illuſion, and upon this is founded the ſyſtem of pre-eſtabliſhed harmony, the difficulties of which I have already pointed out.

It is neceſſary then to take care that we be not entangled in this labyrinth of abſurdities. If you make a ſingle falſe ſtep over the threſhold, you are involved beyond the power of eſcaping. Every thing depends on the firſt ideas formed of extenſion; and the manner in which the partiſans of the ſyſtem of monads endeavour to eſtabliſh it, is extremely ſeductive.

Theſe philoſophers love not to ſpeak of the extenſion of bodies, becauſe they clearly foreſee, that it muſt become fatal to them in the ſequel; but inſtead of ſaying, that bodies are extended, they denominate them compound beings, which no one can deny, as extenſion neceſſarily ſuppoſes diviſibility, and conſequently a combination of parts which conſtitute bodies. But they preſently make a wrong uſe of this notion of a compound being. For, ſay they, a being can be compounded only ſo far as it is made up of ſimple beings; and hence they conclude, that every body is compounded of ſimple beings. As ſoon as you grant them this concluſion, you are caught, beyond the power of retreating; for you are under the neceſſity of admitting, that theſe ſimple beings, not being compounded, are not extended.

This captious argument is exceedingly ſeductive. If you permit yourſelf to be dazzled with it, they have gained their point. Only admit this propoſition, bodies are compounded of ſimple beings, that is, of parts which have no extenſion, and you are entangled. With all your might, then, reſiſt this aſſertion: *every compound being is made up of ſimple beings*; and though you may not be able directly to prove the fallacy, the abſurd conſequences which immediately reſult, would be ſufficient to overthrow it.

In effect, they admit that bodies are extended; from this point the partiſans of the ſyſtem of monads ſet out, to eſtabliſh the propoſition that they are compound beings; and having hence deduced, that bodies are compounded of ſimple beings, they are obliged to allow, that ſimple beings are incapable of producing real extenſion, and conſequently, that the extenſion of bodies is mere illuſion.

An argument whoſe concluſion is a direct contradiction of the premiſes is ſingularly ſtrange: this reaſoning ſets out with advancing that bodies are extended; for, if they were not, how could it be known that they are compound beings, and then comes the concluſion, that they are not ſo. Never was a fallacious argument, in my opinion, more completely refuted than this has been. The queſtion was, *Why are bodies extended?* And, after a little turning and winding, it is anſwered, *Becauſe they are not ſo.* Were I to be aſked, Why has a triangle three ſides? and I ſhould reply, that it is a mere illuſion, would ſuch a reply be deemed ſatisfactory?

It is therefore certain, that this propoſition, 'Every compound being is neceſſarily made up of ſimple beings,' leads to a falſe concluſion, however well-founded it may appear to the partiſans of monads, who even pretend to rank it among the axioms, or firſt principles of human knowledge. The abſurdity in which it immediately iſſues, is ſufficient to overturn it, were there no other reaſons for calling it in queſtion.

But as a compound being here means the ſame thing as an extended being, it is juſt as if it were affirmed, 'Every extended being is compounded of beings which are not ſo.' And this is preciſely the queſtion. It is aſked, Whether, on dividing a body, you arrive at length at parts unſuſceptible of any farther diviſion, for want of extenſion; or, Whether you never arrive at particles ſuch as that the diviſibility ſhould be unbounded?

In order to determine this important queſtion, for the ſake of argument let it be ſuppoſed, that every body is compounded of parts without extenſion. Certain ſpecious reaſonings may eaſily be employed, drawn from the noted principle of the ſufficient reaſon; and it will be ſaid, that a compound being can have its ſufficient reaſon only in the ſimple beings which compoſe it; which might be true, if the compound being were in fact made up of ſimple beings, the very point in queſtion; and whenever this compoſition is denied, the ſufficient reaſon becomes totally inapplicable.

But it is dangerous to enter the liſts with perſons who believe in monads; for, beſides that there is nothing to be gained, they loudly exclaim that you are attacking the principle of the ſufficient reaſon, which is the baſis of all certainty, even of the exiſtence of God. According to them, whoever refuſes to admit monads, and rejects the magnificent fabric, in which every thing is illuſion, is an infidel and an atheiſt. Sure I am that ſuch a frivolous imputation will not make the ſlighteſt impreſſion on your mind, but that you will perceive the wild extravagancies into which men are driven, when they embrace the ſyſtem of monads, a ſyſtem too abſurd to need a refutation in detail; their foundation being abſolutely reduced to a wretched abuſe of the principle of the ſufficient reaſon.

26th May, 1761.

LETTER XVII.

Concluſion of Reflections on this Syſtem.

WE are under the neceſſity of acknowledging the diviſibility of bodies in infinitum, or of admitting the ſyſtem of monads with all the extravagancies reſulting from it; there is no other choice; an alternative which ſupplies the partiſans of that ſyſtem with another formidable argument in ſupport of it.

They pretend that, by diviſibility in infinitum, we are obliged to aſcribe to bodies an infinite quality, whereas it is certain that God alone is infinite.

The partiſans of the ſyſtem of monads are very dangerous perſons; they accuſed us of atheiſm, and now they charge us with polytheiſm, alleging that we aſcribe to all bodies infinite perfections. Thus we ſhould be much worſe than pagans, who only worſhip certain idols, whereas we are accuſed of paying homage to all bodies as ſo many divinities. A dreadful imputation, no doubt, were it well founded; and I ſhould certainly prefer embracing the ſyſtem of monads, with all the chimeras and illuſions which flow from it, to a declaration in favour of diviſibility in infinitum, if it involved a concluſion ſo impious.

You will allow that to reproach one's adverſaries with atheiſm or idolatry is a very ſtrange mode of arguing; but where do they find us aſcribing to bodies this divine infinity? Are they infinitely powerful, wiſe, good, or happy? By no means: we only affirm, that on dividing bodies, though the diviſion be carried on ever ſo far, it will always be poſſible to continue it farther, and that you never can arrive at indiviſible particles. It may accordingly be affirmed that the diviſibility of bodies is without *limits*; and it is improper to uſe the term *infinity*, which is applicable to God alone.

I muſt remark at the ſame time, that the word ‘ infinity’ is not ſo dangerous as theſe philoſophers inſinuate. In ſaying, for example, infinitely wicked, nothing is more remote from the perfections of God.

They admit that our ſouls will never have an end, and thus acknowledge an infinity in the duration of the ſoul, without marking the leaſt diſreſpect to the infinite perfections of God. Again, when you aſk them if the extent of the univerſe is bounded, are they very indeciſive in their anſwer? Some of them frankly allow, that the extent of the univerſe may very probably be infinite, without our being able, however far our ideas are carried, to determine its limits. Here then is one infinity more, which they do not deem heretical.

For a ſtill ſtronger reaſon diviſibility in infinitum ought not to give them the leaſt offence. To be diviſible to infinity is not ſurely an attribute which any one could ever think of aſcribing to the Supreme Being, and confers not on bodies a degree of perfection which would not be far from that which theſe philoſophers allow them, in compounding them of monads, which, on their ſyſtem, are beings endowed with qualities ſo eminent, that they heſitate not to give to God himſelf the denomination of monad.

In truth, the idea of a diviſion which may be continued without any bounds, contains ſo little of the character of the Deity, that it rather places bodies in a rank far inferior to that which ſpirits and our ſouls occupy; for it may well be affirmed, that a ſoul, in its eſſence, is infinitely more valuable than all the bodies in the world. But, on the ſyſtem of monads, every body, even the vileſt, is compounded of a vaſt number of monads, whoſe nature has a great reſemblance to that of our ſouls. Each monad repreſents to itſelf the whole world as eaſily as our ſouls; but, ſay they, their ideas of it are very obſcure, though we have already clear, and ſometimes alſo diſtinct, ideas of it.

But what aſſurance have they of this difference? Is it not to be apprehended that the monads which compoſe the pen wherewith I am writing, may have ideas of the univerſe much clearer than thoſe of my ſoul? How can I be aſſured of the contrary? I ought to be aſhamed to employ a pen in conveying my feeble conceptions, while the monads of which it conſiſts poſſibly conceive much more ſublimely; and you might have greater reaſon to be ſatisfied, ſhould the pen commit its own thoughts to paper, inſtead of mine.

In the ſyſtem of monads, that is not neceſſary; the ſoul repreſents to itſelf, beforehand, by its inherent powers, all the ideas of my pen, but in a very obſcure manner. What I am now taking the liberty to ſuggeſt, contributes abſolutely nothing to your information. The partiſans of this ſyſtem have demonſtrated that ſimple beings cannot exerciſe the ſlighteſt influence on each other; and your own ſoul derives from itſelf what I have been endeavouring to convey, without my having any concern in the matter.

Converſation, reading and writing, therefore, are merely chimerical and deceptive formalities, which illuſion would impoſe upon us as the means of acquiring and extending knowledge. But I have already had the honour of pointing out to you the wonderful conſequences reſulting from the ſyſtem of the pre-eſtabliſhed harmony; and I am apprehenſive that theſe reveries may have become too ſevere a trial of your patience, though many perſons of ſuperior illumination conſider this ſyſtem as the moſt ſublime production of human underſtanding, and are incapable of mentioning it but with the moſt profound reſpect.

I flatter myſelf that I have guarded you ſufficiently againſt ſuch chimeras, however ſeductive their appearance; I ſhould be ſorry, at the ſame time, to have injured in your good opinion a conſiderable part of our modern philoſophers. They are, for the moſt part, extremely innocent, but remain obſtinately attached to the ſyſtem which at firſt impoſed on them, without greatly troubling themſelves about the abſurd conſequences which flow from it.

30th May, 1761.

LETTER XVIII.

Elucidation reſpecting the Nature of Colours.

I AM under the neceſſity of acknowledging, that the ideas reſpecting colour, which I have already taken the liberty to ſuggeſt,* come far ſhort of that degree of evidence to which I could have wiſhed to carry them. This ſubject has hitherto proved a ſtumbling-block to philoſophers, and I muſt not flatter myſelf with the belief that I am able to clear it of every difficulty. I hope, at the ſame time, that the elucidations which I am going to ſubmit to your examination, may go far toward removing a conſiderable part of them.

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* Vol. I. Letters XXVII. XXVIII. and XXXI.

The ancient philoſophers ranked colours among the bodies of which we know only the names. When they were aſked, for example, why ſuch a body was red, they anſwered, it was in virtue of a quality which made it appear red. You muſt be ſenſible that ſuch an anſwer conveys no information, and that it would have been quite as much to the purpoſe to confeſs ignorance.

Deſcartes, who firſt had the courage to plunge into the myſteries of nature, aſcribes colours to a certain mixture of light and ſhade, which laſt being nothing elſe but a want of light, as it is always found where the light does not penetrate, muſt be incapable of producing the different colours we obſerve.

Having remarked that the ſenſations of the organ of ſight are produced by the rays which ſtrike that organ, it neceſſarily follows, that thoſe which excite in it the ſenſation of red, muſt be of quite a different nature from thoſe which produce the ſenſation of the other colours; hence it is eaſily comprehended that each colour is attached to a certain quality of the rays which ſtrike the organ of viſion. A body appears to us red, when the rays which it emits are of a nature to excite in our eyes the ſenſation of that colour.

The whole, then, reſults in an enquiry into the difference of the rays which variety of colours produces. This difference muſt be great, to produce ſo many particular ſenſations in our eyes. But wherein can it conſiſt? This is the great queſtion, toward the ſolution of which our preſent reſearch is directed.

The firſt difference between rays which preſents itſelf is, that ſome are ſtronger than others. It cannot be doubted that thoſe of the ſun, or of any other body very brilliant, or very powerfully illuminated, muſt be much ſtronger than thoſe of a body feebly illuminated, or endowed with a ſlender degree of light; our eyes are aſſuredly ſtruck in a very different manner by the one and by the other.

Hence it might be inferred, that different colours reſult from the force of the rays of light; ſo that the moſt powerful rays ſhould produce, for example, red; thoſe which are leſs ſo, yellow; and in progreſſion, green, and blue.

But there is nothing more eaſy than to overturn this ſyſtem, as we know from experience that the ſame body always appears to be of the ſame colour, be it leſs or more illuminated, or whether its rays be ſtrong or feeble. A red body, for example, appears equally red, expoſed to the brighteſt luſtre of the ſun, and in the ſhade, where the rays are extremely faint. We muſt not then look for the cauſe of the difference of colour in the different degrees of the force of rays of light, it being poſſible to repreſent the ſame colour as well by very forcible as by very faint rays. The feebleſt glimmering ſerves equally well to diſcover to us difference of colours, as the brighteſt effulgence.

It is abſolutely neceſſary, therefore, that there ſhould be another difference of rays diſcovered, which may characterize their nature relatively to the different colours. You will undoubtedly conclude, that in order to diſcover this difference, we muſt be better acquainted with the nature of luminous rays; in other words, we muſt know what it is that, reaching our eyes, renders bodies viſible: this definition of a ray muſt be the juſteſt, as in effect it is nothing elſe but that which enters into the eye by the pupil, and excites the ſenſation in it.

I have already informed you, that there are only two ſyſtems or theories which pretend to explain the origin and nature of rays of light. The one is that of *Newton*, who conſiders them as emanations proceeding from the ſun and other luminous bodies; and the other, that which I have endeavoured to demonſtrate, and of which I have the reputation of being the author, though others have had nearly the ſame ideas of it. Perhaps I may have ſucceeded better than they, in carrying it to a higher degree of evidence. It will be of importance, then, to ſhew, in both ſyſtems, on what principle the difference of colours may be eſtabliſhed.

In that of emanation, which ſuppoſes the rays to iſſue from luminous bodies, in the form of rivers, or rather of fountains, ſpouting out a fluid in all directions,

tions, it is alleged that the particles of light differ in ſize or in ſubſtance, as a fountain might emit wine, oil, and other liquids; ſo that the different colours are occaſioned by the diverſity of the ſubtile matter which emanates from luminous bodies. Red would be, accordingly, a ſubtile matter iſſuing from the luminous body, and ſo of yellow and the other colours. This explication would exhibit clearly enough the origin of the different colours, if the ſyſtem itſelf had a ſolid foundation. I ſhall enter into the ſubject more at large in my next letter.

2d June, 1761.

LETTER XIX.

Reflections on the Analogy between Colours and Sounds.

YOU will be pleaſed to recollect the objections I offered to the ſyſtem of the emanation of light.* They appear to me ſo powerful, as completely to overturn that ſyſtem. I have accordingly ſucceeded in my endeavours to convince certain natural philoſophers of diſtinction, and they have embraced my ſentiments of the ſubject with expreſſions of ſingular ſatisfaction.

Rays of light, then, are not an emanation from the ſun and other luminous bodies, and conſiſt not of a ſubtile matter emitted forcibly by the ſun, and tranſmitted to us with a rapidity which may well fill you with aſtoniſhment. If the rays employed only eight minutes in their courſe from the ſun to us, the torrent would be terrible, and the maſs of that luminary, however vaſt, muſt ſpeedily be exhauſted.

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* Vol. I. Letters XVII. and XVIII.

According to my ſyſtem, the rays of the ſun, of which we have a ſenſible perception, do not proceed immediately from that luminary; they are only particles of ether floating around us, to which the ſun communicates nearer and nearer a motion of vibration, and conſequently they do not greatly change their place in this motion.

This propagation of light is performed in a manner ſimilar to that of ſound. A bell, whoſe ſound you hear, by no means emits the particles which enter your ears. You have only to touch it when ſtruck, to be aſſured that all its parts are in a very ſenſible agitation. This agitation immediately communicates itſelf to the more remote particles of air, ſo that all receive from it ſucceſſively a ſimilar motion of vibration, which, reaching the ear, excite in it the ſenſation of ſound. The ſtrings of a muſical inſtrument put the matter beyond all doubt; you ſee them tremble, go and come. It is even poſſible to determine by calculation how often in a ſecond each ſtring vibrates; and this agitation, being communicated to the particles of air adjacent to the organ of hearing, the ear is ſtruck by it preciſely as often in a ſecond. It is the perception of this tremulous agitation which conſtitutes the nature of ſound. The greater the number of vibrations, produced by the ſtring in a ſecond, the higher or ſharper is the ſound. Vibrations leſs frequent produce lower notes.

We find the circumſtances, which accompany the ſenſation of hearing, in a manner perfectly analogous, in that of ſight.

The medium only, and the rapidity of the vibrations differ. In ſound, it is the air through which the vibrations of ſonorous bodies are tranſmitted. But with reſpect to light, it is the ether, or that medium incomparably more ſubtile and more elaſtic than air, which is univerſally diffuſed wherever the air and groſſer bodies leave interſtices.

As often then as this ether is put into a ſtate of vibration, and is tranſmitted to the eye, it excites in it the ſentiment of viſion, which is, in that caſe, nothing but a ſimilar tremulous motion, whereby the ſmall nervous fibres at the bottom of the eye are agitated.

You eaſily comprehend, that the ſenſation muſt be different, according as this tremulous agitation is more or leſs frequent; or according as the number of vibrations performed in a ſecond is greater or leſs. Hence there muſt reſult a difference ſimilar to that which takes place in ſounds, when the vibrations are more or leſs frequent. This difference is clearly perceptible by the ear, as the character of ſounds in reſpect of flat and ſharp depends on it. You will recollect that the note marked C in the harpſicord performs about 100 vibrations in a ſecond; note D 112; note E 125; note F 133; note G 150; note A 166; note B 187; and note *C* 200. Thus the nature of ſounds depends on the number of vibrations performed in a ſecond.

It cannot be doubted that the ſenſe of ſeeing may be likewiſe differently affected, according as the number of vibrations of the nervous fibres of the bottom of the eye is greater or leſs. When theſe fibres vibrate 1000 times in a ſecond, the ſenſation muſt be quite different from what it would be, did they vibrate 1200 or 1500 times in the ſame ſpace.

True it is that the organ of viſion is not in a condition to reckon numbers ſo great, ſtill leſs than the ear is to reckon the vibrations which conſtitute ſound; but it is always in our power to diſtinguiſh between the greater and the leſs.

In this difference, therefore, we muſt look for the cauſe of difference of colour; and it is certain that each of them correſponds to a certain number of vibrations, by which the fibres of our eyes are ſtruck in a ſecond, though we are not as yet in a condition to determine the number correſponding to each particular colour, as we can do with reſpect to ſounds.

Much reſearch muſt have been employed before it was poſſible to aſcertain the numbers correſponding to all the notes of the harpſichord, though there was an antecedent conviction that their difference was founded on the diverſity of thoſe numbers. Our knowledge reſpecting theſe objects is nevertheleſs conſiderably advanced, from our being aſſured that there prevails a harmony ſo delightful between the different notes of the harpſichord and the different colours; and that the circumſtances of the one ſerve to elucidate thoſe of the other. This analogy accordingly furniſhes the moſt convincing proofs in ſupport of my ſyſtem. But I have reaſons ſtill more ſolid to adduce, which will ſecure it from every attack.

6th June, 1761.

LETTER XX.

Continuation.

NOTHING is more adapted to the communication of knowledge reſpecting the nature of viſion, than the analogy diſcoverable, almoſt in every particular, between it and the hearing. Colours are to the eye what ſounds are to the ear. They differ from each other as flat and ſharp notes differ. Now we know that flat and ſharp in ſounds depends on the number of vibrations whereby the organ of hearing is ſtruck in a given time, and that the nature of each is determined by a certain number which marks the vibrations performed in a ſecond. From this I conclude, that each colour is likewiſe reſtricted to a number of vibrations, which act on viſion; with this difference, that the vibrations which produce ſound reſide in groſs air, whereas thoſe of light and colours are tranſmitted through a medium incomparably more ſubtile and elaſtic. The ſame thing holds as to the objects of both ſenſes. Thoſe of hearing are all of them bodies adapted to the tranſmiſſion of ſound, that is ſuſceptible of a motion of vibration, or of a tremulous agitation, which, communicating itſelf to the air, excites in the organ the ſenſation of a ſound correſponding to the rapidity of the vibrations.

Such are all muſical inſtruments; and, to confine myſelf principally to the harpſichord, we aſcribe to each ſtring a certain ſound which it produces when ſtruck. Thus one ſtring is named C, another D, and ſo on. A ſtring is named C, when its ſtructure and tenſion are ſuch, that being ſtruck, it produces about 100 vibrations in a ſecond; and if it produced leſs or more in the ſame time, it would have the name of a different note, higher or lower.

You will pleaſe to recollect, that the ſound of a ſtring depends on three things, its length, its thickneſs, and the degree of tenſion; the more it is ſtretched, the ſharper its ſound becomes: and as long as it preſerves the ſame diſpoſition, it emits the ſame ſound; but that changes as ſoon as the other undergoes any variation.

Let us apply this to bodies which are the objects of viſion. The minuter particles which compoſe the tiſſue of their ſurface, may be conſidered as ſtrings diſtended, in as much as they are endowed with a certain degree of elaſticity and bulk, ſo that being ſtruck they acquire a motion of vibration, of which they will finiſh a certain number in a ſecond: and on this number depends the colour which we aſcribe to ſuch body. It is red, when the particles of its ſurface have ſuch a degree of tenſion, that being agitated, they perform preciſely ſo many vibrations in a ſecond a ſecond as are neceſſary to excite in us the ſenſation of that colour. A degree of tenſion which would produce vibrations more or leſs rapid, would excite that of a different colour, and then the body would be yellow, green, or blue, &c.

We have not as yet acquired the ability of aſſigning to each colour the number of vibrations which conſtitute its eſſence; we do not ſo much as know which are the colours that require a greater or leſs rapidity of vibration, or rather, it is not yet determined what colours correſpond with high or low notes. It is ſufficient to know, that each colour is attached to a certain number of vibrations, though it has not hitherto been aſcertained; and that you have only to change the tenſion or elaſticity of the particles which form the ſurface of a body, to make it change colour.

We ſee that the moſt beautiful colours in flowers quickly change and diſappear, from a failure of the nutritive juices: and becauſe their particles loſe their vigour or their tenſion. This too is obſervable in every other change of colour.

To place this in a clearer light, let us ſuppoſe that the ſenſation of red requires ſuch a rapidity of vibration, that 1000 are performed in a ſecond; that orange require 1125, yellow 1250, green 1333, blue 1·500, and violet 1666. Though theſe numbers are only ſuppoſed, this affects not the object I have in view. What I ſay as to theſe numbers, will apply in like manner to the really correſponding numbers, if ever they are diſcovered.

A body, then, will be red, when the particles of its ſurface put in vibration, complete 1000 in a ſecond; another body will be orange, when diſpoſed ſo as to complete 1125 in a ſecond, and ſo on. Hence it is obvious that there muſt be an endleſs variety of intermediate colours, between the ſix principal which I have mentioned; and it is likewiſe evident, if the particles of a body being agitated ſhould perform 1400 vibrations in a ſecond, it would be of an intermediate colour between green and blue; green correſponding to number 1333, and blue to 1500.

9th June, 1761.

LETTER XXI.

How opaque Bodies are rendered viſible.

YOU will find no difficulty in the definition I have been giving of coloured bodies. The particles of their ſurface are always endowed with a certain degree of elaſticity, which renders them ſuſceptible of a motion of vibration, as a ſtring is always ſuſceptible of a certain ſound; and it is the number of vibrations which theſe particles are capable of making in a ſecond, which determines the ſpecies of colour.

If the particles of the ſurface have not elaſticity ſufficient to admit of ſuch agitation, the body muſt be black, this colour being nothing elſe but a deprivation of light, and all bodies from which no rays are tranſmitted to our eyes appearing black.

I now come to a very important queſtion, reſpecting which ſome doubts may be entertained. It may be aſked, What is the cauſe of the motion of vibration which conſtitutes the colours of bodies?

Into the diſcovery of this indeed the whole is reſolved; for as ſoon as the particles of bodies ſhall be put in motion, the ether diffuſed through the air will immediately receive a ſimilar agitation, which, continued to our eyes, conſtitutes there that which we call *rays*, from which viſion proceeds.

I remark, firſt, that the particles of bodies are not put in motion by an internal, but an external power, juſt as a ſtring diſtended would remain for ever at reſt, were it not put in motion by ſome external force. Such is the caſe of all bodies in the dark; for, as we ſee them not, it is a certain proof that they emit no rays, and that their particles are at reſt. In other words, during the night, bodies are in the ſame ſtate with the ſtrings of an inſtrument that is not touched, and which emit no ſound; whereas bodies rendered viſible may be compared to ſtrings which emit ſound.

And as bodies become viſible as ſoon as they are illuminated, that is as ſoon as the rays of the ſun, or of ſome other luminous body, fall upon them, it muſt follow, that the ſame cauſe which illuminates them, muſt excite their particles to generate rays, and to produce in our eyes the ſenſation of viſion. The rays of light, then, falling upon a body, put its particles into a ſtate of vibration.

This appears at firſt ſurprizing, becauſe on expoſing our hands to the ſtrongeſt light, no ſenſible impreſſion is made on them. It is to be conſidered, that the ſenſe of touch is in us too groſs to perceive theſe ſubtile and ſlight impreſſions, but that the ſenſe of ſight, incomparably more delicate, is powerfully affected by them; this furniſhes an inconteſtable proof that the rays of light which fall upon a body poſſeſs ſufficient force to act upon the minuter particles, and to communicate to them a tremulous agitation. And in this preciſely conſiſts the action neceſſary to explain how bodies, when illuminated, are put in a condition themſelves to produce rays, by means of which they become viſible to us. It is ſufficient that bodies ſhould be luminous or expoſed to the light, in order to the agitation of their particles, and thereby to their producing themſelves rays which render them viſible to us.

The perfect analogy between hearing and ſight, gives to this explanation the higheſt degree of probability. Let a harpſichord be expoſed to a great noiſe, and you will ſee that not only the ſtrings in general are put into a ſtate of vibration, but you will hear the ſound of each, almoſt as if it were actually touched. The mechaniſm of this phenomenon is eaſily comprehended, as ſoon as it is known that a ſtring agitated is capable of communicating to the air the ſame motion of vibration which, tranſmitted to the ear, excites in it the ſenſation of the ſound which that ſame ſtring emits.

Now as a ſtring produces in the air ſuch a motion, it follows, that the air reciprocally acts on the ſtring, and gives it a tremulous motion. And as a noiſe is capable of putting in motion the ſtrings of a harpſichord, and of extracting ſounds from them, the ſame thing muſt take place in the objects of viſion.

Coloured bodies are ſimilar to the ſtrings of a harpſichord, and the different colours to the different notes, in reſpect of high and low. The light which falls on theſe bodies, being analogous to the noiſe to which the harpſichord is expoſed, acts on the particles of their ſurface, as that noiſe acts on the ſtrings of the harpſichord, and theſe particles thus put in vibration, will produce the rays which ſhall render the body viſible.

This elucidation ſeems to me ſufficient to diſſipate every doubt relating to my theory of colours. I flatter myſelf at leaſt, that I have eſtabliſhed the true principle of all colours, as well as explained how they become viſible to us only by the light whereby bodies are illuminated, unleſs ſuch doubts turn upon ſome other point which I have not touched upon.

13th June, 1761.

LETTER XXII.

The Wonders of the Human Voice.

IN explaining the theory of ſounds, I conſidered only two reſpects in which ſounds could differ: the one regarded the force of ſound, and I remarked that it is greater in proportion as the vibrations excited in the air are more violent. Thus the noiſe of a diſcharge of cannon, or the ringing of a bell, has more force than that of a ſtring, or of the human voice.

The other difference of ſounds is totally independent of this, and refers to flat and ſharp, according to which we ſay ſome are low and others high. My remark relatively to this difference, made it to depend on the number of vibrations completed in a certain given time, ſay a ſecond; ſo that the greater ſuch number is, the higher or ſharper is the ſound, and the ſmaller it is, the ſound is lower or flatter.

You can eaſily comprehend how the ſame note may be either ſtrong or faint; accordingly we ſee that the *fortè* and *piano* employed by muſicians, change in no reſpect the nature of ſounds. Among the good qualities of a harpſichord, it is required that all the notes ſhould have nearly the ſame degree of ſtrength, and it is always conſidered as a great fault when ſome of the ſtrings are wound up to a greater degree of force than the reſt. Now the flat and the ſharp are referable only to the ſimple ſounds, whoſe vibrations follow regularly, and at equal intervals; and, in muſic, we employ only thoſe ſounds which are denominated ſimple. Accords are compound ſounds, or the concourſe of ſeveral produced at once, among the vibrations of which a certain order muſt predominate, which is the foundation of harmony. But when no relation among the vibrations is perceptible, it is a confuſed noiſe, with which it is impoſſible to ſay what note of the harpſichord is in tune, ſuch as the report of a cannon or muſket.

There is ſtill another remarkable difference among the ſimple ſounds, which ſeems to have eſcaped the attention of philoſophers. Two ſounds may be of equal force, and in accord with the ſame note of the harpſichord, and yet very different to the ear. The ſound of a flute is totally different from that of the French-horn, though both may be in tune with the ſame note of the harpſichord, and equally ſtrong; each ſound derives a certain peculiarity from the inſtrument which emits it, but it is impoſſible to deſcribe wherein this conſiſts; the ſame ſtring too emits different ſounds according as it is ſtruck, touched or pinched. You can eaſily diſtinguiſh the ſound of the horn, the flute, and other muſical inſtruments.

The moſt wonderful diverſity, to ſay nothing of the variety of articulation in ſpeech, is obſervable in the human voice, that aſtoniſhing maſter-piece of the Creator. Reflect but for a moment on the different vowels which the mouth ſimply pronounces or ſings. When the vowel *a* is pronounced or ſung, the ſound is quite different from that of *e*, *i*, *o*, *u*, or *ai* pronounced or ſung, though on the ſame tone. We muſt not, then, look for the reaſon of this difference in the rapidity or order of the vibrations; no inveſtigation of philoſophers has hitherto unfolded this myſtery.

You muſt be perfectly ſenſible, that in order to utter theſe different vowels, a different conformation muſt be given to the cavity of the mouth, and that in man the organization of this part is much better adapted to produce theſe effects, than that of animals.

mals. We find accordingly, that certain birds which learn to imitate the human voice, are never capable of diſtinctly pronouncing the different vowels; the imitation is, at beſt, extremely imperfect.

In many organs there is a ſtop which bears the name of the human voice; it uſually, however, contains only the notes which expreſs the vocal ſounds *ai* or *ae*. I have no doubt, that with ſome change it might be poſſible to produce likewiſe the other vocal ſounds *a*, *e*, *i*, *o*, *u*, *ou*; but even this would not be ſufficient to imitate a ſingle word of the human voice; how combine them with the conſonants, which are ſo many modifications of the vowels? We are ſo conformed, that, however common the practice, it is almoſt impoſſible to trace and explain the real mechaniſm.

We diſtinctly obſerve three organs employed in expreſſing the conſonants, the lips, the tongue, and the palate; but the noſe likewiſe eſſentially concurs. On ſtopping it, we become incapable of pronouncing the letters *m* and *n*; the ſound of *b* and *d* only is then to be heard. A ſtriking proof of the marvellous ſtructure of our mouth for the pronunciation of the letters undoubtedly is, that all the ſkill of man has not hitherto been capable of producing a piece of mechaniſm that could imitate it. The ſong has been exactly imitated, but without any articulation of ſounds, and without diſtinction of the different vowels.

The conſtruction of a machine capable of expreſſing ſounds, with all the articulations, would no doubt be a very important diſcovery. Were it poſſible to execute ſuch a piece of mechaniſm, and bring it to ſuch perfection, that it could pronounce all words, by means of certain ſtops, like thoſe of an organ or harpſichord, every one would be ſurprized, and juſtly, to hear a machine pronounce whole diſcourſes or ſermons together, with the moſt graceful accompaniments. Preachers and other orators, whoſe voice is either too weak or diſagreeable, might play their ſermons or orations on ſuch a machine, as organiſts do pieces of muſic. The thing does not ſeem to me impoſſible.

16th June, 1761.

LETTER XXIII.

A Summary of the principal Phenomena of Electricity.

THE ſubject which I am now going to recommend to your attention almoſt terrifies me. The variety it preſents is immenſe, and the enumeration of facts ſerves rather to confound than to inform. The ſubject I mean is Electricity, which, for ſome time paſt, has become an object of ſuch importance in phyſics, that every one is ſuppoſed to be acquainted with its effects.

You muſt undoubtedly have frequently heard it mentioned in converſation; but I know not whether you have ever witneſſed any of the experiments. Natural philoſophers of modern times proſecute the ſtudy of it with ardour, and are almoſt every day diſcovering new phenomena, the deſcription of which would employ many hundreds of letters; nay, perhaps, I ſhould never have done.

And here it is I am embarraſſed. I could not bear to think of letting you remain unacquainted with a branch of natural philoſophy ſo eſſential; but I would willingly ſave you the fatigue of wading through a diffuſe detail of the phenomena, which, after all, would not furniſh the neceſſary information. I flatter myſelf, however, that I have diſcovered a road which will lead ſo directly to the object, that you ſhall attain a knowledge of it much more perfect than that of moſt natural philoſophers, who devote night and day to the inveſtigation of theſe myſteries of nature.

Without ſtopping to explain the various appearances and effects of electricity, which would engage me in a long and tedious detail, without extending your knowledge of the cauſes which produce theſe effects, I ſhall purſue quite a different courſe, and begin with unfolding the true principle of nature on which all theſe phenomena are founded, however various they may appear, and from which they are all eaſily deducible.

It is ſufficient to remark, in general, that electricity is excited by the friction of a glaſs tube. It thereby becomes electrical: and then it alternately attracts and repels light bodies which are applied to it, and on the application of other bodies, ſparks of fire are mutually extracted, which, increaſed in ſtrength, kindle ſpirits of wine and other combuſtible ſubſtances. On touching ſuch tube with the finger, you feel, beſide the ſpark, a puncture which may, in certain circumſtances, be rendered ſo acute as to produce a commotion through the whole body.

Inſtead of a tube of glaſs, we likewiſe employ a globe of the ſame, which is made to turn round an axis, like a turning-wheel. During this motion it is rubbed with the hand, or with a cuſhion applied to it; then the globe becomes electric, and produces the ſame phenomena as the tube.

Beſides glaſs, reſinous bodies, ſuch as Spaniſh wax and ſulphur, likewiſe poſſeſs the property of becoming electric by friction; but certain ſpecies of bodies only have this quality, of which glaſs, ſealing-wax, and ſulphur, are the principal.

With no effect do other bodies undergo friction; no ſign of electricity appears: but on applying them to the firſt, when rendered electric, they immediately acquire the ſame property. They become electric, then, by communication, as the touch, and frequently the approximation only, of electric bodies, renders them ſuch.

All bodies, therefore, are diviſible into two claſſes; in the one are included ſuch as become electric by friction, in the other thoſe which are rendered ſuch by communication, whereas friction produces no manner of effect on them. It is very remarkable, that bodies of the firſt claſs receive no electricity from communication: when you apply to a tube or globe of glaſs ſtrongly electrified, other glaſſes, or bodies, which friction renders electric, this touch communicates no electricity to them. The diſtinction of theſe two claſſes of bodies is worthy of attention, the one claſs being diſpoſed to become electrical by friction only, and not by communication, the other, on the contrary, only by communication.

All metals belong to this laſt claſs, and the communication extends ſo far, that on preſenting one extremity of a wire to an electric body, the other extremity becomes ſo likewiſe, be the wire ever ſo long; and on applying ſtill another wire to the farther extremity of the firſt, the electricity is conveyed through the whole extent of that ſecond thread, and thus electricity may be tranſmitted to the moſt remote diſtances.

Water is a ſubſtance which receives electricity by communication. Large pools have been electrified to ſuch a degree, that an application of the finger has elicited ſparks, and excited pain.

The prevailing perſuaſion now is, that lightning and thunder are the effect of the electricity which the clouds acquire, from whatever cauſe. A thunder ſtorm exhibits the ſame phenomena of electricity, on the great ſcale, which the experiments of natural philoſophers do in miniature.

20th June, 1761.

LETTER XXIV.

The true Principle of Nature, on which are founded all the Phenomena of Electricity.

THE ſummary I have exhibited of the principal phenomena of electricity, has no doubt excited a curioſity to know what occult powers of nature are capable of producing effects ſo ſurprizing.

The greateſt part of natural philoſophers acknowledge their ignorance in this reſpect. They appear to be ſo dazzled by the endleſs variety of phenomena which every day preſent themſelves, and by the ſingularly marvellous circumſtances which accompany theſe phenomena, that they are diſcouraged from attempting an inveſtigation of the true cauſe of them. They readily admit the exiſtence of a ſubtile matter, which is the primary agent in the production of the phenomena, and which they denominate the electric fluid; but they are ſo embarraſſed about determining its nature and properties, that this important branch of phyſics is rendered only more perplexed by their reſearches.

There is no room to doubt, that we muſt look for the ſource of all the phenomena of electricity only in a certain fluid and ſubtile matter; but we have no need to go to the regions of imagination in queſt of it. That ſubtile matter denominated *ether*, whoſe reality I have already endeavoured to demonſtrate,

ſtrate,* is ſufficient very naturally to explain all the ſurprizing effects which electricity preſents. I hope I ſhall be able to ſet this in ſo clear a light, that you ſhall be able to account for every electrical phenomenon, however ſtrange an appearance it may aſſume.

The great requiſite is to have a thorough knowledge of the nature of ether. The air which we breathe riſes only to a certain height above the ſurface of the earth; the higher you aſcend, the more ſubtile it becomes, and at laſt it entirely ceaſes. We muſt not affirm, that beyond the region of the air there is a perfect vacuum, which occupies the immenſe ſpace in which the heavenly bodies revolve. The rays of light which are diffuſed in all directions from theſe heavenly bodies, ſufficiently demonſtrate that thoſe vaſt ſpaces are filled with a ſubtile matter.

If the rays of light are emanations forcibly projected from luminous bodies, as ſome philoſophers have maintained, it muſt follow, that the whole ſpace of the heavens is filled with theſe rays, nay that they move through it with incredible rapidity. You have only to recollect the prodigious velocity with which the rays of the ſun are tranſmitted to us. On this hypotheſis, not only would there be no vacuum, but all ſpace would be filled with a ſubtile matter, and that in a ſtate of conſtant and moſt dreadful agitation.

But I think I have clearly proved, that rays of light are no more emanations projected from luminous

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* Vol. I. Letter XV.

nous bodies, than ſound is from ſonorous bodies. It is much more certain, that rays of light are nothing elſe but a tremulous motion or agitation of a ſubtile matter, juſt as ſound conſiſts of a ſimilar agitation excited in the air. And as ſound is produced and tranſmitted by the air, light is produced and tranſmitted by that matter, incomparably more ſubtile, denominated ether, which conſequently fills the immenſe ſpace between the heavenly bodies.

Ether then is a medium proper for the tranſmiſſion of rays of light, and this ſame quality puts us in a condition to extend our knowledge of its nature and properties. We have only to reflect on the properties of air, which render it adapted to the reception and tranſmiſſion of ſound. The principal cauſe is its elaſticity or ſpring. You know that air has a power of expanding itſelf in all directions, and that it does expand, the inſtant that obſtacles are removed. The air is never at reſt, but when its elaſticity is every where the ſame; whenever it is greater in one place than another, the air immediately expands. We likewiſe diſcover by experiment, that the more the air is compreſſed, the more its elaſticity increaſes: hence the force of air-guns, in which the air, being very ſtrongly compreſſed, is capable of diſcharging the ball with aſtoniſhing velocity. The contrary takes place when the air is rarefied: its elaſticity becomes leſs in proportion as it is more rarefied, or diffuſed over a larger ſpace.

On the elaſticity of the air, then, relative to its denſity, depends the velocity of ſound, which makes a progreſs of about 1000 feet in a ſecond. If the elaſticity of the air were increaſed, its denſity remaining the ſame, the velocity of ſound would increaſe: and the ſame thing would take place if the air were more rare, or leſs denſe than it is, its elaſticity being the ſame. In general, the more that any medium, ſimilar to air, is elaſtic, and at the ſame time leſs denſe, the more rapidly will the agitations excited in it be tranſmitted. And as light is tranſmitted ſo many thouſand times more rapidly than ſound, it muſt clearly follow, that the ether, that medium whoſe agitations conſtitute light, is many thouſand times more elaſtic than air, and, at the ſame time, many thouſand times more rare or more ſubtile, both of theſe qualities contributing to accelerate the propagation of light.

Such is the reaſon which leads to conclude, that ether is many thouſand times more elaſtic and more ſubtile than air; its nature being in other reſpects ſimilar to that of air, in as much as it is likewiſe a fluid matter, and ſuſceptible of compreſſion and of rarefaction. It is this quality which will conduct us to the explanation of all the phenomena of electricity.

23d June, 1761.

LETTER XXV.

Continuation. Different Nature of Bodies relatively to Electricity.

ETHER being a ſubtile matter, and ſimilar to air, but many thouſand times more rare and more elaſtic, it cannot be at reſt, unleſs its elaſticity, or the force with which it tends to expand, be the ſame every where.

As ſoon as the ether in one place ſhall be more elaſtic than in another, which is the caſe when it is more compreſſed there, it will expand itſelf into the parts adjacent, compreſſing what it finds there, till the whole is reduced to the ſame degree of elaſticity. It is then in equilibrio; the equilibrium being nothing elſe but the ſtate of reſt, when the powers which have a tendency to diſturb it counterbalance each other.

When therefore the ether is not in equilibrio, the ſame thing muſt take place as in air, when its equilibrium is diſturbed; it muſt expand itſelf from the place where its elaſticity is greater, toward that where it is leſs; but conſidering its greater elaſticity and ſubtilty, this motion muſt be much more rapid than that of air. The want of equilibrium in the air produces wind, or the motion of that fluid from one place to another. There muſt therefore be produced a ſpecies of wind, but incomparably more ſubtile, than that of air, when the equilibrium of the ether is diſturbed, by which this laſt fluid will paſs from places where it was more compreſſed and more elaſtic, to thoſe where it was leſs ſo.

This being laid down, I with confidence affirm, that all the phenomena of electricity are a natural conſequence of want of equilibrium in the ether, ſo that wherever the equilibrium of the ether is diſturbed, the phenomena of electricity muſt take place; conſequently, electricity is nothing elſe but a derangement of the equilibrium of the ether.

In order to unfold all the effects of electricity, we muſt attend to the manner in which ether is blended and inveloped with all the bodies which ſurround us. Ether, in theſe lower regions, is to be found only in the ſmall interſtices which the particles of the air and of other bodies leave unoccupied. Nothing can be more natural than that the ether, from its extreme ſubtilty and elaſticity, ſhould inſinuate itſelf into the ſmalleſt pores of bodies, which are impervious to air, and even into thoſe of the air itſelf. You will recollect that all bodies, however ſolid they may appear, are full of pores; and many experiments inconteſtibly demonſtrate, that theſe interſtices occupy much more ſpace than the ſolid parts; finally, the leſs ponderous a body is, the more it muſt be filled with theſe pores, which contain ether only. It is clear, therefore, that, though the ether be thus diffuſed through the ſmalleſt pores of bodies, it muſt however be found in very great abundance in the vicinity of the earth.

You will eaſily comprehend, that the difference of theſe pores muſt be very great, both as to magnitude and figure, according to the different nature of the bodies, as their diverſity probably depends on the diverſity of their pores. There muſt be, therefore, undoubtedly, pores more cloſe, and which have leſs communication with others; ſo that the ether which they contain is likewiſe more confined, and cannot diſengage itſelf but with great difficulty, though its elaſticity may be much greater than that of the ether which is lodged in the adjoining pores. There muſt be, on the contrary, pores abundantly open, and of eaſy communication with the adjacent pores; in this caſe it is evident, that the ether lodged in them can with leſs difficulty diſengage itſelf than in the preceding; and if it is more or leſs elaſtic in theſe than in the others, it will ſoon recover its equilibrium.

In order to diſtinguiſh theſe two claſſes of pores, I ſhall denominate the firſt *cloſe*, and the others *open*. Moſt bodies muſt contain pores of an intermediate ſpecies, which it will be ſufficient to diſtinguiſh by the terms *more* or *leſs cloſe*, and *more* or *leſs open*.

This being laid down, I remark, firſt, that if all bodies had pores perfectly cloſe, it would be impoſſible to change the claſticity of the air contained in them; and even though the ether in ſome of theſe pores ſhould have acquired, from whatever cauſe, a higher degree of elaſticity than the others, it would always remain in that ſtate, and never recover its equilibrium, from a total want of communication. In this caſe, no change could take place in bodies;

all would remain in the ſame ſtate as if the ether were in equilibrio, and no phenomenon of electricity could be produced.

This would likewiſe be the caſe if the pores of all bodies were perfectly open: for then, though the ether might be more or leſs elaſtic in ſome pores than in others, the equilibrium would be inſtantly reſtored, from the entire freedom of communication, and that ſo rapidly, that we ſhould not be in a condition to remark the ſlighteſt change. For the ſame reaſon, it would be impoſſible to diſturb the equilibrium of the ether contained in ſuch pores; as often as the equilibrium might be diſturbed, it would be as inſtantaneouſly reſtored, and no ſign of electricity would be diſcoverable.

The pores of all bodies being neither perfectly cloſe nor perfectly open, it will always be poſſible to diſturb the equilibrium of the ether which they contain: and when this happens, from whatever cauſe, the equilibrium cannot fail to re-eſtabliſh itſelf: but this re-eſtabliſhment will require ſome time, and this produces certain phenomena: and you will preſently ſee, much to your ſatisfaction, that they are preciſely the ſame which electrical experiments have diſcovered. It will then appear, that the principles on which I am going to eſtabliſh the theory of electricity are extremely ſimple, and at the ſame time abſolutely incontrovertible.

27th June, 1761.

LETTER

LETTER XXVI.

On the ſame Subject.

I HOPE I have now ſurmounted the moſt formidable difficulties which preſent themſelves in the theory of electricity. You have only to preſerve the idea of ether, which I have been explaining; and which is that extremely ſubtile and elaſtic matter diffuſed not only through all the void ſpaces of the univerſe, but through the minuteſt pores of all bodies, in which it is ſometimes more and ſometimes leſs engaged, according as they are more or leſs cloſe. This conſideration conducts us to two principal ſpecies of bodies, of which the one has pores more cloſe, and the other pores more open.

Should it happen, therefore, that the ether contained in the pores of bodies, has not throughout the ſame degree of elaſticity, and that it is more or leſs compreſſed in ſome than in others, it will make an effort to recover its equilibrium; and it is preciſely from this that the phenomena of electricity take their riſe, which, of conſequence, will be varied, in proportion as the pores in which the ether is lodged are various, and grant it a communication more or leſs free with the others.

This difference in the pores of bodies perfectly correſponds to that which the firſt phenomena of electricity have made us to remark in them, by which ſome eaſily become electrical by communication, or the proximity of an electrical body, whereas others ſcarcely undergo any change. Hence you will immediately infer, that bodies which receive electricity ſo eaſily, by communication alone, are thoſe whoſe pores are open; and that the others which are almoſt inſenſible to electricity, muſt have theirs cloſe, either entirely or to a very great degree.

It is, then, by the phenomena of electricity themſelves, that we are enabled to conclude what are the bodies whoſe pores are cloſe or open. Reſpecting which permit me to ſuggeſt the following elucidations.

Firſt, the air which we breathe has its pores almoſt entirely cloſe; ſo that the ether which it contains, cannot diſengage itſelf but with difficulty, and muſt find equal difficulty in attempting to penetrate into it. Thus, though the ether diffuſed through the air is not in equilibrio with that which is contained in other bodies, where it is more or leſs compreſſed, the re-eſtabliſhment of its equilibrium is not to be produced without extreme difficulty; this is to be underſtood of dry air, humidity being of a different nature, as I ſhall preſently remark.

Farther, we muſt rank in this claſs of bodies, with cloſe pores, *glaſs*, *pitch*, *reſinous bodies*, *ſealing wax*, *ſulphur*, and particularly *ſilk*. Theſe ſubſtances have their pores ſo very cloſe, that it is with extreme difficulty the ether can either eſcape from, or penetrate into, them.

The other claſs, that of bodies whoſe pores are open, contains, firſt, water and other liquors, whoſe nature is totally different from that of air. For this reaſon when air becomes humid, it totally changes its nature with reſpect to electricity, and the ether can enter or eſcape without almoſt any difficulty. To this claſs of bodies, with open pores, likewiſe muſt be referred thoſe of animals, and all metals.

Other bodies, ſuch as wood, ſeveral ſorts of ſtones and earths, occupy an intermediate ſtate between the two principal ſpecies which I have juſt mentioned, and the ether is capable of entering or eſcaping with more or leſs facility, according to the nature of each ſpecies.

After theſe elucidations on the different nature of bodies, with reſpect to the ether which they contain, you will ſee with much ſatisfaction, how all the phenomena of electricity, which have been conſidered as ſo many prodigies, flow very naturally from them.

All depends, then, on the ſtate of the ether, diffuſed or diſperſed through the pores of all bodies, in as far as it has not throughout the ſame degree of elaſticity, or as it is more or leſs compreſſed in ſome than in others; for the ether not being then in equilibrio, will make an effort to recover it. It will endeavour to diſengage itſelf as far as the openneſs of the pores will permit, from places where it is too much compreſſed, to expand itſelf and enter into pores where there is leſs compreſſion, till it is throughout reduced to the ſame degree of compreſſion and elaſticity, and is, of conſequence, in equilibrio.

Let it be remarked, that when the ether paſſes from a body where it was too much compreſſed, into another where it is leſs ſo, it meets with great obſtacles in the air which ſeparates the two bodies, on account of the pores of this fluid, which are almoſt entirely cloſe. It however paſſes through the air, as a liquid and extremely ſubtile matter, provided its force is not inferior, or the interval between the bodies too great. Now this paſſage of the ether being very much impeded, and almoſt entirely prevented by the pores of the air, the ſame thing will happen to it, as to air forced with velocity through ſmall apertures, a hiſſing ſound is heard, which proves that this fluid is then put into an agitation which produces ſuch ſound.

It is, therefore, extremely natural, that the ether, forced to penetrate through the pores of the air, ſhould likewiſe receive a ſpecies of agitation. You will pleaſe to recollect, that as agitation of the air produces ſound, a ſimilar agitation of ether produces light. As often, then, as ether eſcapes from one body, to enter into another, its paſſage through the air muſt be accompanied with light; which appears ſometimes under the form of a ſpark, ſometimes under that of a flaſh of lightning, according as its quantity is more or leſs conſiderable.

Here, then, is the moſt remarkable circumſtance which accompanies moſt electrical phenomena, explained to a demonſtration, on the principles I have laid down. I ſhall now enter into a more particular detail, which will furniſh me with a very agreeable ſubject for ſome following letters.

30th June, 1761.

LETTER

LETTER XXVII.

Of poſitive and negative Electricity. Explanation of the Phenomenon of Attraction.

YOU will eaſily comprehend from what I have above advanced, that a body muſt become electrical, whenever the ether contained in its pores becomes more or leſs elaſtic than that which is lodged in adjacent bodies. This takes place when a greater quantity of ether is introduced into the pores of ſuch body, or when part of the ether which it contained is forced out. In the former caſe, the ether becomes more compreſſed, and conſequently more elaſtic; in the other, it becomes rarer, and loſes its elaſticity. In both caſes, it is no longer in equilibrio with that which is external; and the efforts which it makes to recover its equilibrium, produce all the phenomena of electricity.

You ſee then that a body may become electric in two different ways, according as the ether contained in its pores becomes more or leſs elaſtic than that which is external; hence reſult two ſpecies of electricity: the one, by which the ether is rendered more elaſtic, or more compreſſed, is denominated *increaſed* or *poſitive electricity*; the other, in which the ether is leſs elaſtic, or more rarefied, is denominated *diminiſhed* or *negative electricity*. The phenomena of both are nearly the ſame; a ſlight difference only is obſervable, which I ſhall mention.

greater diameter of the tube, as from the very nature of glaſs.

Experiments give us reaſon to conclude, that the tube of glaſs becomes, by theſe means, ſurcharged with ether, whereas ſealing-wax is exhauſted of it; the phenomena however are nearly the ſame.

It muſt be obſerved, that the glaſs tube retains its electricity as long as it is ſurrounded only with air, becauſe the pores of the glaſs and thoſe of the air, are too cloſe to allow a communication ſufficiently free to the ether, and to exhauſt the glaſs of what it has more than in its natural ſtate; ſuperfluity of ether always increaſing elaſticity. But the air muſt be very dry, for only when in that ſtate are its pores ſufficiently cloſe; when it is humid, or loaded with vapours, experiments do not ſucceed, whatever degree of friction you beſtow on the glaſs. The reaſon is obvious; for water, which renders the air humid, having its pores very open, receives every inſtant the ſuperfluous ether which was in the glaſs, and which of courſe remains in its natural ſtate. Experiments ſucceed, then, in only very dry air; let us now ſee what phenomena a glaſs tube will, in that caſe, produce, *(plate II. fig. 25.)* after having undergone conſiderable friction.

It is clear, that on preſenting to it a ſmall light body *C* with open pores, ſuch as gold-leaf, the ether in the tube more elaſtic at the neareſt parts *D*, *E*, will not make ineffectual efforts to diſcharge itſelf and paſs into the pores of the body *C*. It will force a path through the air, provided the diſtance be not

too

.II Continuation of the Figures of Plate II Second Series

Fig 23

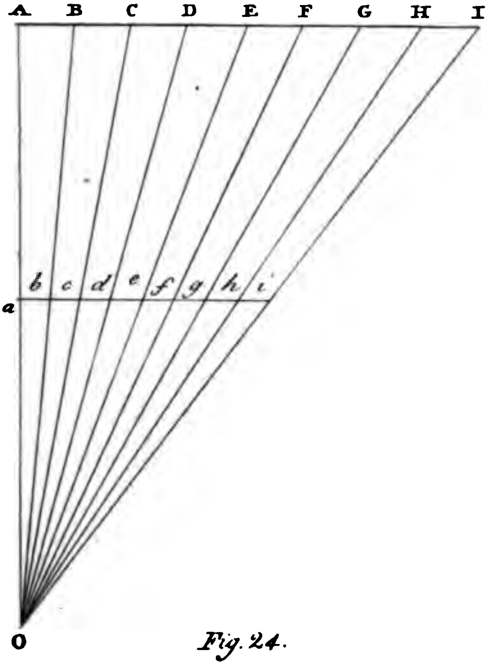


Fig. 24.

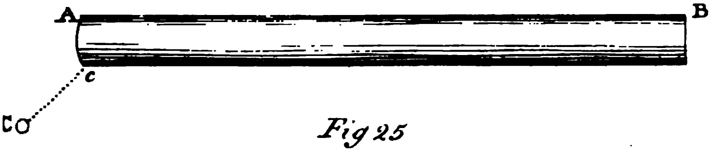
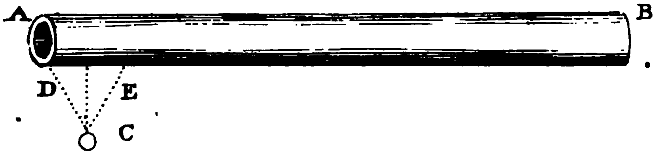


Fig 25



too great; and you will even ſee a light between the tube and the body occaſioned by the agitation excited in the ether, which paſſes with difficulty from the tube into the body. When, inſtead of the body C, the finger is applied to it, you feel a pricking occaſioned by the rapid entrance of the ether; and if you expoſe your face to it at ſome diſtance, you feel a certain agitation in the air, excited by the tranſition of the ether. Theſe circumſtances are likewiſe accompanied, ſometimes, with a ſlight cracking, produced undoubtedly by the agitation of the air which the ether traverſes with ſuch rapidity.

I muſt entreat you to keep in mind, that an agitation in the air always produces a ſound, and that the motion of ether produces light; and then the explanation of theſe phenomena will become abundantly eaſy.

Let the ſmall light body *C*, be replaced in the vicinity of our electric tube; as long as the ether is eſcaping from the tube, to enter into the pores of the body *C*, the air will be in part expelled from it, and conſequently will not preſs ſo ſtrongly on the body on that ſide, as in every other direction; it will happen, then, as in the preceding caſe, that the body *C* will be impelled toward the tube, and, being light, will come cloſe up to it. We ſee, then, that this apparent attraction equally takes place, whether the ether in the tube be too much or too little elaſtic; or, whether the elaſticity of the tube be poſitive or negative. In both caſes, the paſſage of the ether ſtops the air, and by its preſſure hinders it from acting.

But while the ſmall body *C* is approaching the tube, the paſſage of the ether becomes ſtronger, and the body *C* will ſoon be as much ſurcharged with ether as the tube itſelf. Then the action of the ether, which ariſes from its elaſticity only, entirely ceaſes, and the body *C* will ſuſtain on all ſides an equal preſſure. The attraction will ceaſe, and the body *C* will remove from the tube, as nothing detains it, and its own gravity puts it in motion. Now as ſoon as it removes, its pores being open, its ſuperfluous ether preſently eſcapes in the air, and it returns to its natural ſtate. The body will then act as at the beginning, and you will ſee it again approach the tube, ſo that it will appear alternately attracted and repelled by it; and this play will go on till the tube has loſt its electricity. For as, on every attraction, it diſcharges ſome portion of its ſuperfluous ether, beſides the inſenſible eſcape of part of it in the air, the tube will ſoon be re-eſtabliſhed in its natural ſtate, and in its equilibrium; and this ſo much the more ſpeedily as the tube ſhall be ſmall, and the body *C* light: then all the phenomena of electricity will ceaſe.

7th July, 1761.

LETTER XXIX.

On the electric Atmoſphere.

I HAD almoſt forgot to bring forward a moſt eſſential circumſtance, which accompanies all electric bodies, whether *poſitively* or *negatively* ſuch, and which ſupplies ſome very ſtriking elucidations for explaining the phenomena of electricity.

Though it be indubitably true that the pores of air are very cloſe, and ſcarcely permit any communication between the ether that they contain, and what is in the vicinity, it undergoes however ſome change when near to an electric body.

Let us firſt conſider an electric body *negatively* ſo, as a ſtick of ſealing-wax *A B*, (*plate III. fig.* 1.) which has been deprived by friction of part of the ether contained in its pores, ſo that what it now contains has leſs elaſticity than that of other bodies, and conſequently than that of the air which ſurrounds the wax. It muſt neceſſarily happen, that the ether contained in the particles of the air which immediately touch the wax, as at *m*, having greater elaſticity, ſhould diſcharge itſelf, in however ſmall a degree, into the pores of the wax, and will conſequently loſe ſomewhat of its elaſticity. In like manner, the particles of air more remote, ſuppoſe at *n*, will likewiſe ſuffer a portion of their either to eſcape into the nearer at *m*, and ſo on to a certain diſtance, beyond which the air will no longer undergo any change. In this manner, the air round the ſtick of wax, to a certain diſtance, will be deprived of part of its ether, and become itſelf electric.

This portion of the air, which thus partakes of the electricity of the ſtick of wax, is denominated the *electric atmoſphere*; and you will ſee from the proofs which I have juſt adduced, that every electric body muſt be ſurrounded with an atmoſphere. For if the body is *poſitively* electric, ſo as to contain a ſuperfluity of ether, it will be more compreſſed in ſuch a body, and conſequently more elaſtic, as is the caſe with a tube of glaſs when rubbed; this ether, more elaſtic, then diſcharges itſelf, in a ſmall degree, into the particles of air which immediately touch it, and thence into particles more remote, to a certain diſtance; this will form another electric atmoſphere round the tube, in which the ether will be more compreſſed, and conſequently more elaſtic than elſewhere.

It is evident that this atmoſphere which ſurrounds ſuch bodies, muſt gradually diminiſh the electricity of them, as in the firſt caſe there paſſes almoſt continually a ſmall portion of ether, from the ſurrounding air, into the electric body, and which, in the other caſe, iſſues from the electric body, and paſſes into the air. This is likewiſe the reaſon why electric bodies at length loſe their electricity; and this ſo much the ſooner, as the pores of the air are more open. In a humid air, whoſe pores are very open, all electricity is almoſt inſtantly extinguiſhed; but in very dry air it continues a conſiderable time.

This electric atmoſphere becomes abundantly ſenſible, on applying your face to an electrified body; you have a feeling ſimilar to the application of a ſpider's web, occaſioned by the gentle tranſition of the ether from the face into the electric body, or reciprocally from this laſt into the face, according as it is negative or poſitive, to uſe the common expreſſion.

The electric atmoſphere ſerves likewiſe more clearly to explain that alternate attraction and repulſion of light bodies placed near to electric bodies, which I mentioned in the preceding letter; in which you muſt have remarked, that the explanation of repulſion, there given, is incomplete; but the electric atmoſphere will ſupply the defect.

Let *A B (plate III. fig. 2.)* repreſent an electric tube of glaſs ſurcharged with ether, and let *C* be a ſmall light body, with pores ſufficiently open, in its natural ſtate. Let the atmoſphere extend as far as the diſtance *D E*. Now, as the vicinity of *C* contains already an ether more elaſtic, this will diſcharge itſelf into the pores of the body *C*, there will immediately iſſue from the tube a new ether, which will paſs from *D* into *C*, and it is the atmoſphere chiefly which facilitates this paſſage. For if the ether contained in the air had no communication with that in the tube, the corpuſcle *C* would not feel the vicinity of the tube; but while the ether is paſſing from *D* to *C*, the preſſure of the air between *C* and *D* will be diminiſhed, and the corpuſcle *C* will no longer be preſſed equally in all directions; it will therefore be impelled toward *D*, as if attracted by it. Now in proportion as it approaches, it will be likewiſe more and more ſurcharged with ether, and will become electric as the tube itſelf, and, conſequently, the electricity of the tube will no longer act upon it.

But as now the corpuſcle, being arrived at *D*, contains too much ether, and more than the air at *E*, it will have a tendency to eſcape, in order to make its way to *E*. The atmoſphere, in which the compreſſion of the ether continues to diminiſh from *D* to *E* will facilitate this paſſage, and the ſuperfluous ether will in effect flow from the corpuſcle toward *E*. By this paſſage, the preſſure of the air on the corpuſcle will be ſmaller on that ſide than every where elſe, and conſequently the corpuſcle will be carried toward *D*, as if the tube repelled it. But as ſoon as it arrives at *E*, it diſcharges the ſuperfluous ether, and recovers its natural ſtate; it will then be again attracted toward the tube, and, having reached it, will be again repelled, for the reaſon which I have juſt been explaining.

It is the electric atmoſphere then, chiefly, which produces theſe ſingular phenomena, when we ſee electrified bodies alternately attract and repel ſmall light bodies, ſuch as little ſlips of paper, or particles of metal, with which this experiment beſt ſucceeds, as the ſubſtances have very open pores.

You will ſee, moreover, that what I have juſt now ſaid reſpecting *poſitive* electricity, muſt equally take place in *negative*. The tranſition of the ether is only reverſed, by which the natural preſſure of the air muſt always be diminiſhed.

11th July, 1761.

LETTER

LETTER XXX.

Communication of Electricity to a Bar of Iron, by means of a Globe of Glaſs.

AFTER the experiments made with glaſs tubes, we have proceeded to carry electricity to a higher degree of ſtrength. Inſtead of a tube, a globe, or round ball of glaſs, has been employed, which is made to turn with great velocity round an axis, and on applying the hand to it, or a cuſhion of ſome matter with open pores, a friction is produced, which renders the globe completely electric.

Figure 4. *of plate III.* repreſents this globe, which may be made to move round an axis A B, by a mechaniſm ſimilar to that employed by turners. C is the cuſhion ſtrongly applied to the globe, on which it rubs as it turns round. The pores of the cuſhion being, in this friction, compreſſed more than thoſe of the glaſs, the ether contained in it is expelled, and forced to inſinuate itſelf into the pores of the glaſs, where they continue to accumulate, becauſe the open pores of the cuſhion are continually ſupplying it with more ether, which it is extracting, at leaſt in part, from ſurrounding bodies; and thus the globe may be ſurcharged with ether to a much higher degree than glaſs tubes. The effects of electricity are, accordingly, rendered much more conſiderable, but of the ſame nature with thoſe which I have deſcribed, alternately attracting and repelling light bodies;

dies; and the ſparks which we ſee, on touching the electric globe, are much more lively.

But naturaliſts have not reſted ſatisfied with ſuch experiments, but have employed the electrical globe in the diſcovery of phenomena much more ſurprizing.*

Having conſtructed the machine for turning the globe round its axis AB, a bar of iron FG is ſuſpended above, or on one ſide of, the globe, and toward the globe is directed a chain of iron or other metal ED, terminating at D, in metallic filaments, which touch the globe. It is ſufficient that this chain be attached to the bar of iron in any manner whatever, or but touch it. When the globe is turned round, and, in turning, made to rub on the cuſhion at C, in order that the glaſs may become ſurcharged with ether, which will, conſequently, be more elaſtic, it will eaſily paſs from thence into the filaments D, for, being of metal, their pores are very open; and from thence, again, it will diſcharge itſelf by the chain DE, into the bar of iron FG. Thus by means of the globe, the ether extracted from the cuſhion C, will ſucceſſively accumulate in the bar of iron FG, which likewiſe, of conſequence, becomes electric, and its electricity increaſes in proportion as you continue to turn the globe.

If this bar had a farther communication with other bodies, whoſe pores too are open, it would ſoon diſcharge into them its ſuperfluous ether, and thereby loſe its electricity; the ether extracted from the cuſhion would be diſperſed over all the bodies which had an inter-communication, and its greateſt compreſſion would not be more perceptible. To prevent this, which would prove fatal to all the phenomena of electricity, the bar muſt, of neceſſity, be ſupported, or ſuſpended, by props of a ſubſtance whoſe pores are very cloſe; ſuch are glaſs, pitch, ſulphur, ſealing-wax, and ſilk. It would be proper, then, to ſupport the bar on props of glaſs or pitch; or to ſuſpend it by cords of ſilk. The bar is thus ſecured againſt the tranſmiſſion of its accumulated ether, as it is ſurrounded on all ſides only by bodies with cloſe pores, which grant hardly any admiſſion to the ether in the bar. The bar is then ſaid to be *iſolated*, that is, deprived of all contact which could communicate, and thereby diminiſh, its electricity. You muſt be ſenſible, however, that it is not poſſible abſolutely to prevent all waſte; for this reaſon the electricity of ſuch a bar muſt continually diminiſh, if it were not kept up by the motion of the globe.

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* See Plate III. Fig. 3.

In this manner electricity may be communicated to a bar of iron, which never could be done by the moſt violent and perſevering friction, becauſe of the openneſs of its pores. And, for the ſame reaſon, ſuch a bar rendered electric by communication, produces phenomena much more ſurprizing. On preſenting to it a finger, or any other part of the body, you ſee a very brilliant ſpark dart from it, which, entering into the body, excites a pungent, and ſometimes painful, ſenſation. I recollect my once having preſented to it my head, covered with my peruke and hat, and the ſtroke penetrated it ſo acutely, that I felt the pain next day.

Theſe ſparks, which eſcape from every part of the bar, on preſenting to it a body with open pores, ſet on fire, at once, ſpirit of wine, and kill ſmall birds whoſe heads are expoſed to them. On plunging the end of the chain DE into a baſon filled with water, and ſupported by bodies with cloſe pores, ſuch as glaſs, pitch, ſilk, the whole water becomes electric; and ſome authors aſſure us that they have ſeen conſiderable lakes electrified in this manner, ſo that, on applying the hand, you might have ſeen even very pungent ſparks emitted from the water. But it appears to me, that the globe muſt be turned a very long time indeed, to convey ſuch a portion of ether, into a maſs of water ſo enormous; it would be likewiſe neceſſary that the bed of the lake, and every thing in contact with it, ſhould have their pores cloſe.

The more open, then, the pores of a body are, the more diſpoſed it is to receive a higher degree of electricity, and to produce prodigious effects. You muſt admit that all this is perfectly conformable to the principles which I at firſt eſtabliſhed.

14th July, 1761.

LETTER XXXI.

Electriſation of Men and Animals.

AS electricity may be communicated from glaſs to a bar of iron, by means of a chain, which forms that communication, it may likewiſe be conveyed into the human body; for the bodies of animals have this property in common with metals and water, that their pores are very open; but the man, who is to be the ſubject of the experiment, muſt not be in contact with other bodies whoſe pores are likewiſe open.

For this effect, the man is placed on a large lump of pitch, or ſeated on a chair ſupported by glaſs columns, or a chair ſuſpended by cords of ſilk, as all theſe ſubſtances have pores ſufficiently cloſe to prevent the eſcape of the ether, with which the body of the man becomes ſurcharged by electricity.

This precaution is abſolutely neceſſary, for were the man placed on the ground, the pores of which are abundantly open, as ſoon as the ether was conveyed into his body, to a higher degree of compreſſion, it would immediately diſcharge itſelf into the earth, and we muſt be in a condition to ſurcharge it entirely with ether, before the man could become electric. Now you muſt be ſenſible, that the cuſhion by which the globe of glaſs is rubbed, could not poſſibly ſupply ſuch a prodigious quantity of ether, and that were you to extract it even out of the earth itſelf, you could gain no ground, for you would juſt take away as much on the one hand as you gave on the other.

Having, then, placed the man, whom you mean to electrify, in the manner which I have indicated, you have only to make him touch, with his hand, the globe of glaſs while it turns, and the ether, accumulated in the globe, will eaſily paſs into the open pores of the hand, and diffuſe itſelf over the whole body, from whence it cannot ſo eaſily eſcape, as the air, and all the bodies with which he is ſurrounded, have their pores cloſe. Inſtead of touching the globe with his hand, it will be ſufficient for him to touch the chain, or even the bar, which I deſcribed in the preceding letter; but in this caſe, not only the man himſelf muſt be ſurcharged with ether, but likewiſe the chain with the bar of iron; and as this requires a greater quantity of ether, it would be neceſſary to labour longer in turning the globe, in order to ſupply a ſufficient quantity.

In this manner the man becomes entirely electric, or, in other words, his whole body will be ſurcharged with ether, and this fluid will conſequently be found there in the higheſt degree of compreſſion or electricity, and will have a violent tendency to eſcape.

You muſt be abundantly ſenſible, that a ſtate ſo violent cannot be indifferent to the man. The body is, in its minuteſt parts, wholly penetrated with ether, and the ſmalleſt fibres, as well as the nerves, are ſo filled with it, that this ether, without doubt, pervades the principal ſprings of animal and vital motion. It is, accordingly, obſerved, that the pulſe of a man electrified beats faſter, he is thrown into a ſweat, and the motion of the more ſubtile fluids, with which the body is filled, becomes more rapid. A certain change is likewiſe felt over the whole body, which it is impoſſible to deſcribe; and there is every reaſon to believe, that this ſtate has a powerful influence on the health, though ſufficient experiments have not yet been made, to aſcertain in what caſes this influence is ſalutary or otherwiſe. It may ſometimes be highly beneficial to have the blood and humours raiſed to a more lively circulation; certain obſtructions, which threaten dangerous conſequences, might thereby be prevented; but on other occaſions, an agitation too violent might prove injurious to health. The ſubject certainly well deſerves the attention of medical gentlemen. We have heard of many ſurprizing cures performed by electricity, but we are not yet enabled ſufficiently to diſtinguiſh the occaſions on which we may promiſe ourſelves ſucceſs.

To return to our electrified man; it is very remarkable that, in the dark, we ſee him ſurrounded with a light, ſimilar to that which painters throw round the heads of ſaints. The reaſon is abundantly obvious; as there is always eſcaping from the body of that man, ſome part of the ether with which he is ſurcharged, this fluid meets much reſiſtance from the cloſe pores of the air, it is thereby put into a certain agitation, which is the origin of light, as I have had the honour to demonſtrate.

Phenomena of a very ſurprizing nature are remarked, in this ſtate of a man electrified. On touching him, you not only ſee very brilliant ſparks iſſue from the part which you touch, but the man feels, beſides, a very pungent pain. Farther, if the perſon who touches him be in his natural ſtate, or not electrified, both ſenſibly feel this pain, which might have fatal conſequences, eſpecially if he were touched in the head, or any other part of the body of acute ſenſibility. You will readily comprehend, then, how little indifferent it is to us, that part of the ether contained in our body eſcape from it, or that new ether is introduced, eſpecially as this is done with ſuch amazing rapidity.

Moreover, the light, with which we ſee the man ſurrounded in the dark, is an admirable confirmation of my remarks reſpecting the electric atmoſphere which is diffuſed round all bodies; and you will no longer find any difficulty in the moſt part of electrical phenomena, however inexplicable they may, at firſt, appear.

18th July, 1761.

LETTER XXXII.

Diſtinctive Character of the two Species of Electricity.

YOU will pleaſe to recollect, that not only glaſs becomes electric by friction, but that other ſubſtances, ſuch as ſealing-wax and ſulphur, have the ſame property, in as much as their pores are likewiſe cloſe, ſo that whether you introduce into them an extraordinary quantity of ether, or extract a part of it, they continue for ſome time in that ſtate, nor is the equilibrium ſo ſoon reſtored.

Accordingly, inſtead of a globe of glaſs, globes of ſealing-wax and ſulphur are employed, which are likewiſe made to revolve round an axis, rubbing at the ſame time againſt a cuſhion, in the ſame manner which I deſcribed reſpecting a globe of glaſs. Such globes are thus rendered equally electric, and, on applying to them a bar of iron which touches them only by ſlender filaments or fringes of metal, incapable of injuring the globe, electricity is immediately communicated to that bar, from which you may afterwards tranſmit it to other bodies at pleaſure.

Here, however, a very remarkable difference is obſervable. A globe of glaſs rendered electric in this manner, becomes ſurcharged with ether, and the bar of iron, or other bodies brought into communication with it, acquire an electricity of the ſame nature; or, in other words, the ether contained is in a ſtate of too great compreſſion, whoſe elaſticity is increaſed. This electricity is denominated *poſitive* or *augmented* electricity. But when a globe of ſealing-wax or ſulphur is treated in the ſame manner; an electricity directly oppoſite is the reſult, which is denominated *negative*, or *diminiſhed* electricity, as it is perceived that, by friction, theſe globes are deprived of part of the ether contained in their pores.

You will be ſurprized to ſee that the ſame friction is capable of producing effects altogether oppoſite; but this depends on the nature of the bodies which undergo the friction, whether by communicating or receiving it, and of the rigidity of their particles which contain the pores. In order to explain the poſſibility of this difference, it is evident, at firſt ſight, that when two bodies are rubbed violently againſt each other, the pores of the one muſt, in moſt caſes, undergo a greater compreſſion than thoſe of the other, and that then, the ether contained in the pores, is extruded, and forced to inſinuate itſelf into thoſe of the bodies which are leſs compreſſed.

It follows, then, that in this friction of glaſs againſt a cuſhion, the pores of the cuſhion undergo a greater compreſſion than thoſe of the glaſs, and conſequently the ether of the cuſhion muſt paſs into the glaſs, and produce in it a poſitive or *increaſed* electricity, as I have already ſhewed. But on ſubſtituting a globe of ſealing-wax or of ſulphur in place of the glaſs, theſe ſubſtances being ſuſceptible of a greater degree of compreſſion in their pores, than the ſubſtance of the cuſhion with which the friction is performed, a part of the ether contained in theſe globes will be forced out, and conſtrained to paſs into the cuſhion; the globe of ſealing-wax or ſulphur will, thereby, be deprived of part of its ether, and, of courſe, receive a negative or *diminiſhed* electricity.

The electricity which a bar of iron, or of any other metal, receives from communication with a globe of ſealing-wax or ſulphur, is of the ſame nature; as is alſo that which is communicated to a man placed on a lump of pitch, or ſuſpended by cords of ſilk. When ſuch a man, or any other body, with open pores, electrified in the ſame manner, is touched, nearly the ſame phenomena are obſervable, as in the caſe of poſitive electricity. The touch is here likewiſe accompanied with a ſpark, and a puncture on both ſides. The reaſon is obvious: for the ether which, in this caſe, eſcapes from bodies in their natural ſtate, to enter into electrified bodies, being under conſtraint, muſt be under an agitation, which produces light. A ſenſible difference is, however, to be remarked in the figure of the ſpark, according as the electricity is poſitive or negative. See that of poſitive electricity: *plate III. fig.* 5.

If the bar A B poſſeſſes poſitive electricity, and the finger C is preſented to it, the light which iſſues out of the bar appears under the form of rays diverging from the bar toward the finger *m n*, and the luminous point is ſeen next the finger.

But if the bar A B *(plate III. fig.* 6.*)* is negatively electric, and the finger C is preſented to it, the luminous rays *m n* diverge from the finger, and you ſee the luminous point *p* next the bar.

This is the principal character by which poſitive is diſtinguiſhed from negative electricity. From whence ſoever the ether eſcapes, the ſpark is emitted in the figure of rays diverging from that point; but when the ether enters into a body, the ſpark is a luminous point toward the recipient body.

21ſt July, 1761.

LETTER XXXIII.

How the ſame Globe of Glaſs may furniſh, at once, the two Species of Electricity.

YOU will be enabled to ſee ſtill more clearly the difference between poſitive and negative electricity, after I have explained how it is poſſible to produce, by one and the ſame globe of glaſs, both the ſpecies; and this will ſerve, at the ſame time, farther to elucidate theſe wonderful phenomena of nature.

Let A B (*plate VI. fig.* 1.) be the globe of glaſs, turning round its axis C, and rubbed againſt by the cuſhion D, in an oppoſite direction to which the globe is touched by the metallic filaments F, attached to the bar of iron F G, which is ſuſpended by cords of ſilk H and I, that it may no where touch bodies with open pores.

This being laid down, you know that, by friction againſt the cuſhion D, the ether paſſes from the cuſhion into the glaſs, from which it becomes more compreſſed, and conſequently more elaſtic: it will paſs, therefore, from thence, by the metallic filaments

F, into

Fig. 1

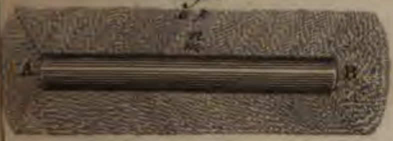


Fig. 2

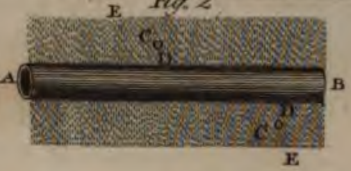
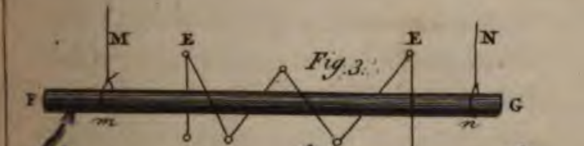
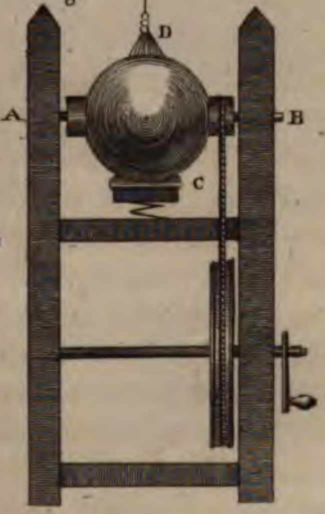


Fig. 3.





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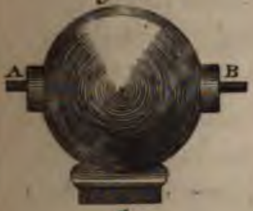

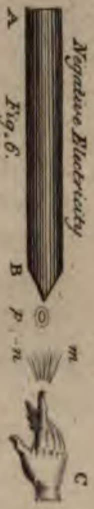

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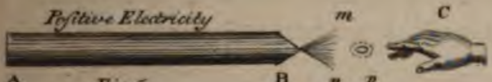

Fig. 5

F, into the bar of iron F G: for though the pores of glaſs are abundantly cloſe, as the ether in the globe is continually accumulating by the friction, it ſoon becomes ſo overcharged, that it eſcapes by the metallic filaments, and diſcharges itſelf into the bar, by which this laſt becomes equally electric.

Hence you perceive, that all this ſuperfluity of ether is ſupplied by the cuſhion, which would ſpeedily be exhauſted, unleſs it had a free communication with the frame which ſupports the machine, and thereby with the whole earth which is every inſtant ſupplying the cuſhion with new ether, ſo that as long as the friction continues, there is a quantity ſufficient farther to compreſs that which is in the globe and in the bar. But if the whole machinery is made to reſt on pillars of glaſs, as M and N, or if it is ſuſpended by cords of ſilk, that the cuſhion may have no communication with bodies whoſe pores are open, which might ſupply the deficiency of ether, it would ſoon be exhauſted, and the electricity could not be conveyed into the globe and the bar, beyond a certain degree, which will be ſcarcely perceptible, unleſs the cuſhion be of a prodigious ſize. To ſupply this defect, the cuſhion D is put in communication with a large maſs of metal E, the ether of which is ſufficient to ſupply the globe and the bar, and to carry it to ſuch a high degree of compreſſion.

You will thus procure to the globe and to the bar a poſitive electricity, as has been already explained. But in proportion as they become ſurcharged with ether, the cuſhion and the metallic maſs E will loſe the ſame quantity, and thereby acquire a negative electricity: ſo that we have here, at once, the two ſpecies of electricity; the poſitive in the bar, and the negative in the metallic maſs. Each produces its effect conformably to its nature. On preſenting a finger to the bar, a ſpark with divergent rays will iſſue from the bar, and the luminous point will be ſeen toward the finger; but if you preſent the finger to the metallic maſs, the ſpark with divergent rays will iſſue from the finger, and you will ſee the luminous point toward the maſs.

Let us ſuppoſe two men placed on lumps of pitch, to cut off all communication between them and bodies with open pores: let the one touch the bar, and the other the metallic maſs, while the machine is put in motion; it is evident that the former will become poſitively electric, or ſurcharged with ether, whereas the other, he who touches the metallic maſs, will acquire a negative electricity, and loſe his ether.

Here then are two electric men, but in a manner totally different, though rendered ſuch by the ſame machine. Both will be ſurrounded by an electric atmoſphere, which, in the dark, will appear like the light that painters throw about the figures of ſaints. The reaſon is, that the ſuperfluous ether of the one inſenſibly eſcapes into the circumambient air; and that, with reſpect to the other, the ether contained in the air inſenſibly inſinuates itſelf into his body. This tranſition, though inſenſible, will be accompanied with an agitation of ether, which produces light.

It is evident that theſe two ſpecies of electricity are directly oppoſite; but in order to have a thorough conviction of it, let theſe two join hands, or only touch each other, and you will ſee very vehement ſparks iſſue from their bodies, and they themſelves will feel very acute pain.

If they were electrified in the ſame manner, which would be the caſe if both touched the bar or the metallic maſs, they might ſafely touch each other; no ſpark, and no pain, would enſue, becauſe the ether contained in both would be in the ſame ſtate; whereas in the caſe laid down, their ſtate is directly oppoſite.

25th July, 1761.

LETTER XXXIV.

The Leyden Experiment.

I NOW proceed to deſcribe a phenomenon of electricity, which has made a great deal of noiſe, and which is known by the name of *the Leyden experiment*, becauſe Mr. *Mouſchenbroeck*, profeſſor at Leyden, is the inventor of it. What is moſt aſtoniſhing in this experiment, is the terrible ſtroke reſulting from it, by which ſeveral perſons at once may receive a very violent ſhock.

Let C be a globe of glaſs, turned round by means of the handle E, and rubbed by the cuſhion DD, which is preſſed upon the globe by the ſpring O. At Q are the metallic filaments, which tranſmit the electricity into the bar of iron FG, by the metallic chain P.

meeting every where, in the body of the man, open pores, will without obſtruction be diffuſed over it.

IV.

Hitherto we ſee only the uſual effect of electricity; but while the ether with ſuch rapidity flies over the body of the man, it diſcharges itſelf with equal rapidity, by the other hand, or by the chain A, into the water in the baſon; and as it enters this with ſuch impetuoſity, it will eaſily overcome the obſtacle oppoſed by the glaſs, and penetrate into the water which the bottle contains.

V.

Now the water in the bottle containing already an ether too much compreſſed; it will acquire, from this increaſe, new force, and will diffuſe itſelf with impetuoſity as well through the chain I H, as through the bar itſelf: it will, of conſequence, make its eſcape thence at *a* with new efforts; and as this is performed in an inſtant, it will enter into the finger with increaſed force, to be diffuſed over the whole body of the man.

VI.

Paſſing thence a-new into the water in the baſon, and penetrating the bottle, it will increaſe ſtill farther the agitation of the ether compreſſed in the water of the bottle, and in the bar; and this will continue till the whole is reſtored to equilibrium, which will quickly take place, from the great rapidity with which the ether acts.

VII. The

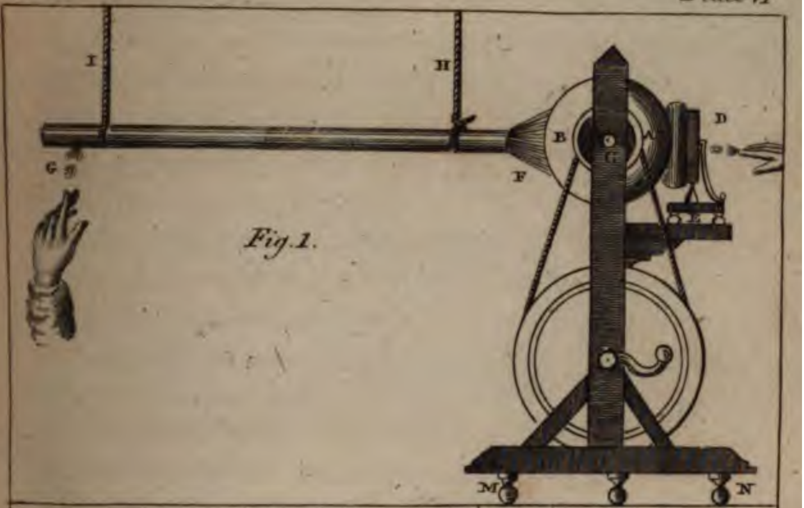

Fig. 1.

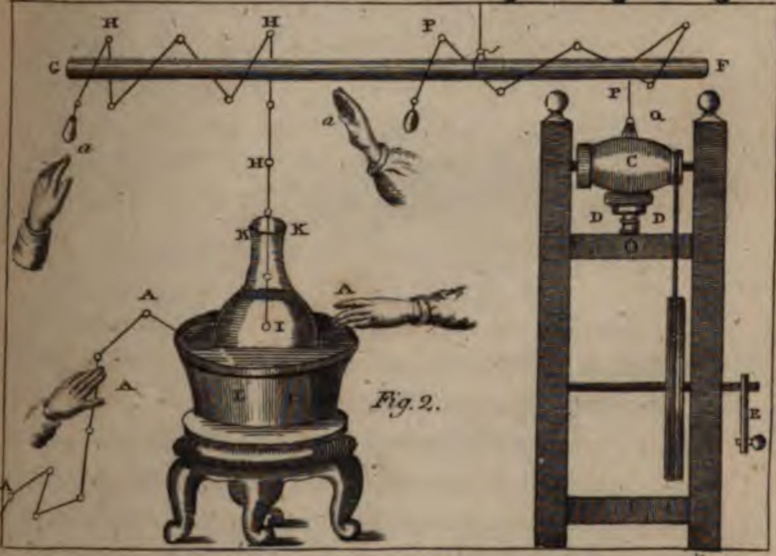

Fig. 2.

VII.

The ſame thing will happen if you employ ſeveral perſons inſtead of one man. And now, I flatter myſelf, you fully comprehend whence ariſes the ſurprizing increaſe of force in the electricity which is produced by this experiment of Mr. *Mouſchenbroeck*, and which exhibits effects ſo prodigious.

VIII.

If any doubt could remain reſpecting what I have advanced, that the ether compreſſed in the water of the bottle could not penetrate through the glaſs, and that afterwards I have allowed it a paſſage abundantly free; ſuch doubt will vaniſh when it is conſidered, that, in the firſt caſe, every thing is in a ſtate of tranquillity, and, in the laſt, the ether is in a terrible agitation, which muſt undoubtedly aſſiſt its progreſs through the cloſeſt paſſages.

28th July, 1761.

LETTER XXXV.

Reflections on the Cauſe and Nature of Electricity, and on other Means proper to produce it.

AFTER theſe elucidations, you can be at no loſs reſpecting the cauſe of the prodigious effects obſervable in the phenomena of electricity.

Moſt authors who have treated the ſubject, perplex the experiments in ſuch a manner, that they are rendered abſolutely unintelligible, eſpecially when they attempt an explanation. They have recourſe to a certain ſubtile matter which they denominate *the electric fluid*, and to which they aſcribe qualities ſo extravagant that the mind rejects them with contempt; and they are conſtrained to acknowledge, at length, that all their efforts are inſufficient to furniſh us with a ſolid knowledge of theſe important phenomena of nature.

But you are enabled to conclude, from the principles which I have unfolded, that bodies evidently become electric, only ſo far as the elaſticity, or the ſtate of the compreſſion of the ether in the pores of bodies, is not the ſame as every where elſe; in other words, when it is more or leſs compreſſed in ſome than in others. For, in that caſe, the prodigious elaſticity with which the ether is endowed, makes violent efforts to recover its equilibrium, and to reſtore every where the ſame degree of elaſticity, as far as the nature of the pores, which, in different bodies, are more or leſs open, will permit; and it is the return to equilibrium which always produces the phenomena of electricity.

When the ether eſcapes from a body where it is more compreſſed, to diſcharge itſelf into another where it is leſs ſo, this paſſage is always obſtructed by the cloſe pores of the air; hence it is put into a certain agitation, or violent motion of vibration, in which, as we have ſeen, light conſiſts; and the more great this motion is, the more brilliant the light becomes, till it becomes at length capable of ſetting them on fire, and of burning them.

While the ether penetrates the air with ſo much force, the particles of air are likewiſe put into a motion of vibration, which occaſions ſound; it is accordingly obſerved, that the phenomena of electricity are accompanied with a cracking noiſe, greater or leſs, according to the diverſity of circumſtances.

And as the bodies of men and animals are filled with ether in their minuteſt pores, and as the action of the nerves ſeems to depend on the ether contained in them, it is impoſſible that men and animals ſhould be indifferent with reſpect to electricity; and when the ether in them is put into a great agitation, the effect muſt be very ſenſible, and, according to circumſtances, ſometimes ſalutary, and ſometimes hurtful. To this laſt claſs, undoubtedly, muſt be referred the terrible ſhocks of the Leyden experiment; and there is every reaſon to believe that it might be carried to a degree of force capable of killing men, for, by means of it, many ſmall animals, ſuch as mice and birds, have actually been killed.

Though friction uſually is employed in the production of electricity, you will eaſily comprehend, that there may be other means beſides this. Whatever is capable of carrying the ether contained in the pores of a body to a greater or leſs degree of compreſſion than ordinary, renders it electric; and if it's pores are cloſe, there the electricity will be of ſome duration; whereas in bodies whoſe pores are open, it cannot poſſibly ſubſiſt, unleſs ſurrounded by air, or other bodies with cloſe pores.

Hence it has been obſerved, that heat frequently ſupplies the place of friction. When you heat or melt ſealing-wax, or ſulphur, in a ſpoon, you diſcover a very ſenſible electricity in theſe ſubſtances, after they are cooled. The reaſon is no longer a myſtery, as we know that heat enlarges the pores of all bodies, for they occupy a greater ſpace when hot than when they are cold.

You know that, in a thermometer, the mercury riſes in heat, and falls in cold; becauſe it occupies a greater ſpace when it is hot, and fills the tube more than when it is cold. We find, for the ſame reaſon, that a bar of iron very hot is always ſomewhat longer than when cold; a property common to all bodies with which we are acquainted.

When, therefore, we melt, by fire, a maſs of ſealing-wax, or ſulphur, their pores are enlarged, and probably more open; a greater quantity of ether muſt, of courſe, be introduced to fill them. When, afterwards, theſe ſubſtances are ſuffered to cool, the pores contract and cloſe, ſo that the ether in them is reduced to a ſmaller ſpace, and conſequently carried to a higher degree of compreſſion, which increaſes it's elaſticity: theſe maſſes will acquire, therefore, a poſitive electricity, and muſt conſequently exhibit the effects of it.

This property of becoming electric by heat is remarked in moſt precious ſtones. Nay there is a ſtone found in Ceylon, named *Tourmalin*, which, rubbed or heated, acquires at once the two ſpecies of electricity. The ether in one part of the ſtone is expelled, to compreſs more that which is in the other part; and its pores are too cloſe to permit the re-eſtabliſhment of the equilibrium.

1ſt Auguſt, 1761.

LETTER XXXVI.

Nature of Thunder: Explanations of the ancient Philoſophers, and of Deſcartes: *Reſemblance of the Phenomena of Thunder, and thoſe of Electricity.*

I HAVE, hitherto, conſidered electricity only as an object of curioſity and ſpeculation to naturaliſts: but you will preſently ſee, not without ſome degree of ſurprize, that thunder and lightning, as well as all the terrible phenomena which accompany them, derive their origin from the ſame principle; and that in theſe, nature executes, on the great ſcale, what naturaliſts do in miniature, by their experiments.

Thoſe philoſophers, who thought they ſaw ſome reſemblance between the phenomena of thunder and thoſe of electricity, were at firſt conſidered as viſionaries; and it was imagined that they made uſe of this pretence merely as a cover to their ignorance reſpecting the cauſe of thunder; but you will ſoon be convinced, that every other explanation of theſe grand operations of nature is deſtitute of foundation.

In truth, every thing advanced on the ſubject, previous to the knowledge of electricity, was a maſs of abſurdity, and little calculated to convey inſtruction reſpecting any, the leaſt, phenomena of thunder.

Ancient philoſophers attributed the cauſe of it to ſulphureous and bituminous vapours, which, aſcending from the earth into the air, mixed with the clouds, where they caught fire, from ſome unknown cauſe.

Deſcartes, who quickly perceived the inſufficiency of this explanation, imagined another cauſe in the clouds themſelves, and thought that thunder might be produced by the ſudden fall of more elevated clouds, on others in a lower region of the air; that the air, contained in the intermediate ſpace, was compreſſed by this fall, to ſuch a degree as was capable of exciting a noiſe ſo loud, and even of producing lightning and thunder, though it was impoſſible for him to demonſtrate the poſſibility of it.

But, without detaining your attention on falſe explanations which lead to nothing, I haſten forward to inform you, that it has been diſcovered by inconteſtable proofs, that the phenomena of thunder are always accompanied by the moſt indubitable marks of electricity.

Let a bar of metal, ſay of iron, be placed on a pillar of glaſs, or any other ſubſtance whoſe pores are cloſe, that when the bar acquires electricity, it may not eſcape, or communicate itſelf to the body which ſupports the bar; as ſoon as a thunder-ſtorm ariſes, and the clouds which contain the thunder come directly over the bar, you perceive in it a very ſtrong electricity, generally far ſurpaſſing that which art produces; if you apply the hand to it, or any other body with open pores, you ſee burſting from it, not only a ſpark, but a very bright flaſh, with a noiſe ſimilar to thunder; the man, who applies his hand to it, receives a ſhock ſo violent that he is ſtunned. This ſurpaſſes curioſity, and there is good reaſon why we ſhould be on our guard, and not approach the bar during a ſtorm.

A profeſſor at Peterſburgh, named *Richmann*, has furniſhed a melancholy example. Having perceived a reſemblance ſo ſtriking between the phenomena of thunder and thoſe of electricity, this unfortunate naturaliſt, the more clearly to aſcertain it by experiment, raiſed a bar of iron on the roof of his houſe, caſed below in a tube of glaſs, and ſupported by a maſs of pitch. To the bar he attached a wire, which he conducted into his chamber, that as ſoon as the bar ſhould become electric, the electricity might have a free communication with the wire, and ſo enable him to prove the effects in his apartment. And it may be proper to inform you, that this wire was conducted in ſuch a manner as no where to be in contact but with bodies whoſe pores are cloſe, ſuch as glaſs, pitch, or ſilk, to prevent the eſcape of the electricity.

Having made this arrangement, he expected a thunder-ſtorm, which, unhappily for him, ſoon came. The thunder was heard at a diſtance; Mr. *Richmann* was all attention to his wire, to ſee if he could perceive any mark of electricity. As the ſtorm approached, he judged it prudent to employ ſome precaution, and not keep too near the wire; but happening careleſsly to advance his cheſt a little, he received a terrible ſtroke, accompanied with a loud clap, which ſtretched him lifeleſs on the floor.

About the ſame time, the late Dr. *Lieberkuhn* and Dr. *Ludolf* were about making ſimilar experiments in this city, and in that view had fixed bars of iron on their houſes; but being informed of the diſaſter which had befallen Mr. *Richmann*, they had the bars of iron immediately removed, and, in my opinion, they acted wiſely.

From this you will readily judge, that the air or atmoſphere muſt become very electric during a thunder-ſtorm, or that the ether contained in it muſt then be carried to a very high degree of compreſſion. This ether, with which the air is ſurcharged, will paſs into the bar, becauſe of it's open pores, and it will become electric, as it would have been in the common method, but in a much higher degree.

4th Auguſt, 1761.

LETTER XXXVII.

Explanation of the Phenomena of Lightning and Thunder.

THE experiments now mentioned inconteſtably demonſtrate, therefore, that ſtormy clouds are [illegible] electrical, and that, conſequently, their [illegible] are either ſurcharged with ether, or exhauſted, [illegible] ſtates are equally adapted to electricity. But [illegible] very powerful reaſons for believing that this [illegible] is poſitive, that the ether in them is compreſſed

preſſed to the higheſt degree, and, conſequently, ſo much the more elaſtic than elſewhere.

Such ſtorms uſually ſucceed very ſultry weather. The pores of the air, and of the vapours floating in it, are then extremely enlarged, and filled with a prodigious quantity of ether, which eaſily takes poſſeſſion of all the empty ſpaces of other ſubſtances. But when the vapours collect in the ſuperior regions of our atmoſphere, there to form clouds, they have to encounter exceſſive cold. Of this it is impoſſible to doubt, from the hail which is frequently formed in theſe regions; this is a ſufficient proof of congelation, as well as the ſnow which we find on the tops of very high mountains, ſuch as the Cordeliers, while extreme heat prevails below.

Nothing then is more certain, or better eſtabliſhed by proof, than the exceſſive cold which univerſally prevails in the upper regions of the atmoſphere, where clouds are formed. It is equally certain; that cold contracts the pores of bodies, by reducing them to a ſmaller ſize: now, as the pores of the vapours have been extremely enlarged by the heat, as ſoon as they are formed aloft into clouds, the pores contract, and the ether which filled them, not being able to eſcape, becauſe thoſe of the air are very cloſe, it needs muſt remain there: it will be of courſe compreſſed to a much higher degree of denſity, and, conſequently, it's elaſticity will be ſo much the greater.

The real ſtate of ſtormy clouds, then, is this, the ether contained in their pores, is much more elaſtic than uſual, or, in other words, the clouds have a poſitive electricity. As they are only an aſſemblage of humid vapours, their pores are very open, but being ſurrounded by the air whoſe pores are cloſe, this ether could not eſcape but very imperceptibly. But if any perſon, or any body whatever, with open pores, were to approach it, the ſame phenomena which electricity exhibits would preſent themſelves; a very vehement ſpark, or rather a real flaſh, would burſt forth. Nay more, the body would undergo a very violent ſhock by the diſcharge, from the impetuoſity with which the ether in the cloud would ruſh into it's pores. This ſhock might be indeed ſo violent as to deſtroy the ſtructure; and, finally, the terrible agitation of the ether which burſts from the cloud, being not only light, but a real fire, it might be capable of kindling and conſuming combuſtible bodies.

Here, then, you will diſtinguiſh all the circumſtances which accompany thunder; and as to the noiſe of thunder, the cauſe is very obvious, for it is impoſſible the ether ſhould be in ſuch a ſtate of agitation, without the air itſelf receiving from it the moſt violent concuſſions, which forcibly impel the particles, and excite a dreadful noiſe. Thunder, then, burſts forth, as often as the force of ether contained in the clouds is capable of penetrating into a body, where the ether is in it's natural ſtate, and whoſe pores are open: it is not even neceſſary that ſuch body ſhould immediately touch the cloud.

What I have ſaid reſpecting the atmoſphere of [illegible] bodies, principally takes place in clouds;

and frequently, during a ſtorm, we are made ſenſible of this electric atmoſphere by a ſtifling air, which is particularly oppreſſive to certain perſons. As ſoon as the cloud begins to diſſolve into rain, the air, becoming humid by it, is charged with an electricity by which the commotion may be conveyed to bodies at a very great diſtance.

It is obſerved that thunder uſually ſtrikes very elevated bodies, ſuch as the ſummits of church-ſpires, when they conſiſt of ſubſtances with open pores, as all metals are; and the pointed form contributes not a little to it. Thunder frequently falls, likewiſe, on water, the pores of which are very open; but bodies with cloſe pores, as glaſs, pitch, ſulphur, and ſilk, are not greatly ſuſceptible of the thunder ſtroke, unleſs they are very much moiſtened. It has been accordingly obſerved, that when thunder paſſes through a window, it does not perforate the glaſs, but always the lead or other ſubſtances which unite the panes. It is almoſt certain, that an apartment of glaſs cemented by pitch, or any other ſubſtance with cloſe pores, would be an effectual ſecurity againſt the ravages of thunder.

8th Auguſt, 1761.

LETTER XXXVIII.

Continuation.

THUNDER, then, is nothing elſe but the effect of the electricity with which the clouds are endowed; and as an electrified body, applied to another in it's natural ſtate, emits a ſpark with ſome noiſe, and diſcharges into it the ſuperfluous ether, with prodigious impetuoſity; the ſame thing takes place in a cloud that is electric, or ſurcharged with ether, but with a force incomparably greater, becauſe of the terrible maſs that is electrified, and in which, according to every appearance, the ether is reduced to a much higher degree of compreſſion than we are capable of carrying it by our machinery.

When, therefore, ſuch a cloud approaches bodies, prepared for the admiſſion of it's ether, this diſcharge muſt be made with incredible violence: inſtead of a ſimple ſpark, the air will be penetrated with a prodigious flaſh, which, exciting a commotion in the ether contained in the whole adjoining region of the atmoſphere, produces a moſt brilliant light; and in this lightning conſiſts.

The air is, at the ſame time, put into a very violent motion of vibration, from which reſults the noiſe of thunder. This noiſe muſt, no doubt, be excited at the ſame inſtant with the lightning; but you know that ſound always requires a certain quantity of time, in order to it's tranſmiſſion to any diſtance, and that its progreſs is only at the rate of about a thouſand feet in a ſecond; whereas light travels with a velocity inconceivably greater. Hence we always hear the thunder later than we ſee the lightning: and from the number of ſeconds intervening between the flaſh and the report, we are enabled to determine the diſtance of the place where it is generated, allowing a thouſand feet to a ſecond.

The body itſelf, into which the electricity of the cloud is diſcharged, receives from it a moſt dreadful ſtroke; ſometimes it is ſhivered to pieces; ſometimes ſet on fire and conſumed, if combuſtible; ſometimes melted, if it be of metal: and, in ſuch caſes, we ſay it is thunder-ſtruck; the effects of which, however ſurprizing and extraordinary they may appear, are in perfect conſiſtency with the well-known phenomena of electricity.

A ſword, it is known, has ſometimes been by thunder melted in the ſcabbard, while the laſt ſuſtained no injury: this is to be accounted for, from the openneſs of the pores of the metal, which the ether very eaſily penetrates, and exerciſes over it all its powers, whereas the ſubſtance of the ſcabbard is more cloſely allied to the nature of bodies with cloſe pores, which permit not to the ether ſo free a tranſmiſſion.

It has likewiſe been found, that of ſeveral perſons, on whom the thunder has fallen, ſome only have been ſtruck by it; and that thoſe who were in the middle ſuffered no injury. The cauſe of this phenomenon likewiſe is manifeſt. In a group expoſed to a thunder a thunder ſtorm, they are in the greateſt danger who ſtand in the neareſt vicinity to the air that is ſurcharged with ether; as ſoon as the ether is diſcharged upon one, all the adjoining air is brought back to its natural ſtate, and conſequently thoſe who were neareſt to the unfortunate victim feel no effect; while others, at a greater diſtance, where the air is ſtill ſufficiently ſurcharged with ether, are ſtruck with the ſame thunder clap.

In a word, all the ſtrange circumſtances, ſo frequently related, of the effects of thunder, contain nothing which may not be eaſily reconciled with the nature of electricity.

Some philoſophers have maintained, that thunder did not come from the clouds, but from the earth, or bodies. However extravagant this ſentiment may appear, it is not ſo abſurd, as it is difficult to diſtinguiſh, in the phenomena of electricity, whether the ſpark iſſues from the body which is electrified, or from that which is not ſo, as it equally fills the ſpace between the two bodies; and if the electricity is negative, the ether and the ſpark are in effect emitted from the natural or non-electrified body. But we are ſufficiently aſſured that, in thunder, the clouds have a poſitive electricity, and that the lightning is emitted from the clouds.

You will be juſtifiable however in aſking, if by every ſtroke of thunder, ſome terreſtrial body is affected? We ſee, in fact, that it very rarely ſtrikes buildings, or the human body: but we know, at the ſame time, that trees are frequently affected by it, and that many thunder-ſtrokes are diſcharged into the earth and into the water. I believe, however, it might be maintained, that a great many do not deſcend ſo low, and that the electricity of the clouds is very frequently diſcharged into the air or atmoſphere.

The ſmall opening of the pores of the air no longer oppoſes any obſtruction to it, when vapours or rain have rendered it ſufficiently humid; for then, we know, the pores open.

It may very poſſibly happen, in this caſe, that the ſuperfluous ether of the clouds ſhould be diſcharged ſimply into the air; and when this takes place, the ſtrokes are neither ſo violent, nor accompanied with ſo great a noiſe, as when the thunder burſts on the earth, when a much greater extent of atmoſphere is put in agitation.

11th Auguſt, 1761.

LETTER XXXIX.

The Poſſibility of preventing, and of averting, the Effects of Thunder.

IT has been aſked, Whether it might not be poſſible to prevent, or to avert, the fatal effects of thunder? You are well aware of the importance of the queſtion, and under what obligation I ſhould lay a multitude of worthy people, were I able to indicate an infallible method of finding protection againſt thunder.

The knowledge of the nature and effects of electricity,

tricity, permits me not to doubt that the thing is poſſible. I correſponded ſome time ago with a Moravian prieſt, named *Procopius Diviſch*, who aſſured me that he had averted, during a whole ſummer, every thunder-ſtorm which threatened his own habitation, and the neighbourhood, by means of a machine conſtructed on the principles of electricity. Several perſons, ſince arrived from that country, have aſſured me that the fact is undoubted, and confirmed by irreſiſtible proof.

But there are many reſpectable characters, who, on the ſuppoſition that the thing is practicable, would have their ſcruples reſpecting the lawfulneſs of employing ſuch a preſervative. The ancient pagans, no doubt, would have conſidered him as impious, who ſhould have preſumed to interfere with Jupiter, in the direction of his thunder. Chriſtians, who are aſſured that thunder is the work of God, and that Divine Providence frequently employs it to puniſh the wickedneſs of men, might with equal reaſon allege, that it were impiety to attempt to oppoſe the courſe of ſovereign juſtice.

Without involving myſelf in this delicate diſcuſſion, I remark that conflagrations, deluges, and many other general calamities, are likewiſe the means employed by Providence to puniſh the ſins of men; but no one, ſurely, ever will pretend, that it is unlawful to prevent, or reſiſt, the progreſs of a fire or an inundation. Hence I infer, that it is perfectly lawful to uſe the means of prevention againſt the effects of thunder, if they are attainable.

The melancholy accident which befel Mr. *Richmann* at Peterſburg, demonſtrates, that the thunder-ſtroke which this gentleman unhappily attracted to himſelf, would undoubtedly have fallen ſome where elſe, and that ſuch place thereby eſcaped; it can therefore no longer remain a queſtion whether it be poſſible to conduct thunder to one place in preference to another; and this ſeems to bring us near our mark.

It would, no doubt, be a matter of ſtill greater importance, to have it in our power to diveſt the clouds of their electric force, without being under the neceſſity of expoſing any one place to the ravages of thunder; we ſhould, in that caſe, altogether prevent theſe dreadful effects, which terrify ſo great a part of mankind.

This appears by no means impoſſible; and the Moravian prieſt, whom I mentioned above, unqueſtionably effected it; for I have been aſſured, that his machinery ſenſibly attracted the clouds, and conſtrained them to deſcend quietly in a diſtillation, without any but a very diſtant thunder-clap.

The experiment of a bar of iron, in a very elevated ſituation, which becomes electric on the approach of a thunder-ſtorm, may lead us to the conſtruction of a ſimilar machine, as it is certain, that in proportion as the bar diſcharges its electricity, the clouds muſt loſe preciſely the ſame quantity; but it muſt be contrived in ſuch a manner, that the bars may immediately diſcharge the ether which they have attracted.

It would be neceſſary, for this purpoſe, to procure for them a free communication with a pool, or with the bowels of the earth, which, by means of their open pores, may eaſily receive a much greater quantity of ether, and diſperſe it over the whole immenſe extent of the earth, ſo that the compreſſion of the ether may not become ſenſible in any particular ſpot. This communication is very eaſy by means of chains of iron, or any other metal, which will, with great rapidity, carry off the ether with which the bars are ſurcharged.

I would adviſe the fixing of ſtrong bars of iron, in very elevated ſituations, and ſeveral of them together, their higher extremity to terminate in a point, as this figure is very much adapted to the attraction of electricity. I would, afterwards, attach long chains of iron to theſe bars, which I would conduct under ground into a pool, lake, or river, there to diſcharge the electricity; and I have no doubt, that after making repeated eſſays, the means may be certainly diſcovered of rendering ſuch machinery more commodious, and more certain in its effect.

It is abundantly evident, that on the approach of a thunder-ſtorm, the ether, with which the clouds are ſurcharged, would be tranſmitted in great abundance into theſe bars, which would thereby become very electric, unleſs the chains furniſhed to the ether a free paſſage, to ſpend itſelf in the water, and in the bowels of the earth.

The ether of the clouds would continue, therefore, to enter quietly into the bars, and would, by its agitation, produce a light, which might be viſible on the pointed extremities.

Such light is, accordingly, often obſerved, during a ſtorm, on the ſummit of ſpires, an infallible proof that the ether of the cloud is there quietly diſcharging itſelf; and every one conſiders this as a very good ſign, of the harmleſs abſorption of many thunder-ſtrokes.

Lights are likewiſe frequently obſerved at ſea, on the tops of the maſts of ſhips, known to ſailors by the name of *Caſtor and Pollux;* and when ſuch ſigns are viſible, they conſider themſelves as ſafe from the ſtroke of thunder.

Moſt philoſophers have ranked theſe phenomena among vulgar ſuperſtitions; but we are now fully aſſured, that ſuch ſentiments are not without foundation; indeed they are infinitely better founded than many of our philoſophic reveries.*

15th Auguſt, 1761.

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* It was deemed improper to divert the reader's attention from *Euler*'s train of thought, on the ſubject of electricity, by notes on particular paſſages, according as the obſervations occurred. But now that the ſubject is cloſed, I am happy to inſert, at once, a few general remarks on *Euler*'s theory: furniſhed by a gentleman well-known in the world of ſcience, both by his literary productions, and by his various and highly improved mathematical inſtruments; and who has lately enriched it by his valuable Lectures on Natural and Experimental Philoſophy, in 5 vols. 8vo. I mean Mr. *George Adams*, of Fleet-ſtreet.—*E. E.*

" However Mr. *Euler* may have deviated from the preſent theories of electricity, in the explanation of particular phenomena, the great outline of his ſyſtem ſeems to be confirmed by the conjectures of *Newton*, the opinions of *Watſon*, *Franklin*, *Wilſon*, *Eeles*, &c." Theſe great men concur in ſuppoſing an etherial fluid to be univerſally diſſeminated through all matter; and every experiment

Proves, that its active properties depend on a ſolution of continuity (which is nearly related to a change in the pores of bodies) whether produced by local motion, rupture of parts, friction, expanſion by heat, or condenſation by cold. Still there is no real ground for ſuppoſing that thoſe ſtates of electricity, called poſitive and negative, depend on the pores of bodies being more or leſs open, for it is highly probable, that the two ſtates are always produced, at the ſame time, in all bodies, and either ſtate may be exhibited by the ſame body, under certain circumſtances, which can have no effect on the pores of the bodies concerned.

It is clear, that all electric appearances depend on an inviſible fluid, of whoſe effects, while it remains in its natural ſtate, we are totally ignorant; but, whenever this ſtate is diſturbed, thoſe appearances which we call electrical take place. Of theſe I ſhall notice a few, ſomewhat more fully than has been done by Mr. *Euler*. Obſerving, firſt, that ſince his time the catalogue of thoſe ſubſtances which tranſmit, with eaſe, the electric fluid, as well as thoſe that do not conduct it, has been conſiderably increaſed; and none have yet been found but what have more or leſs affinity with the electric fluid. It is worthy of remark, that many bodies which are non-conductors when cold, will tranſmit the fluid, when they are heated; a circumſtance which ſeems to confirm the ideas of Mr. *Euler*.

Though Mr. *Euler* has endeavoured to explain the mechaniſm of electric attraction, and repulſion, he has not pointed out the general laws by which it is governed. Theſe are, Firſt, that bodies electrified poſitively repel each other: Secondly, that bodies electrified negatively repel each other: Thirdly, bodies electrified by contrary powers ſtrongly attract each other: Fourthly, bodies that are electrified attract thoſe ſubſtances which are not electrified.

No experiments, on this ſubject, have engaged the attention of philoſophers more than thoſe on the Leyden phial. The myſteries of this experiment are far from being unveiled, and we are in poſſeſſion of no theory that will clearly account for it. Dr. *Franklin* ſhewed, that the Leyden phial could not be charged, unleſs ſome conducting ſubſtance, in contact with the outſide, was alſo connected with the ground, &c. He maintained, that the ſurplus electricity which was received, by one of the coated ſurfaces of the phial, was actually taken from the other; that one ſide was always poſitively, the other always negatively, electrified. This theory has been ſhewn to be defective, and it has been proved that, while the phial is charging, both ſides are electrified poſitively.

By connecting ſeveral bottles together, the electric power may be increaſed almoſt at pleaſure, animals may be be killed, and polarity may be given to magnetic needles.

It has been already obſerved, that the electric fluid, in paſſing from one body to another, emitted rays of light; later experiments have ſhewn that there is ſcarce any ſubſtance, ſolid or fluid, but what may be rendered luminous by paſſing through it the electric fluid. It has alſo been found, in many inſtances, to produce the ſame effects as culinary fire; thus it will raiſe the thermometer, accelerate vegetation, and promote evaporation, &c.

After that aſtoniſhing diſcovery, by which the ſimilarity of Lightning and Electricity was proved, conducting rods were adaped to buildings, to preſerve them from being injured by lightning. Theſe are, in general, allowed to be uſeful, though not ſo extenſively as was at firſt imagined; and, though they may conduct the lightning that falls upon, or meets with, them, ſafely to the ground, yet it is abſurd to ſuppoſe, that a conductor can prevent the lightning from ſtriking a building, or is capable of exhauſting the atmoſphere of its electricity.

One of the moſt brilliant diſcoveries, concerning Electricity, is that of its activity in carrying on the functions of animal life. Mr. *Walſh* has ſhewn, that the powers of the Torpedo, and the Gymnotus Electricus, like thoſe of the Leyden phial, might be extended to any diſtance through a medium of conducting ſubſtances, and that thoſe bodies which impede the diſcharge of an electrical accumulation, had the ſame influence on the powers of the Torpedo, &c. Through a ſmall interval, in the circuit formed to conduct the influence of theſe animals, a ſpark of light has been made to appear, which has fully eſtabliſhed the ſimilitude between the electrical fluid of theſe animals and that of nature at large. Many inſtances might be introduced to ſhew its agency in the human frame, but none can be offered of more importance to mankind, than thoſe which prove its efficacy as a medicine: it has been applied with ſucceſs, to relieve the ſufferings of mankind, and, at a great hoſpital in this metropolis, under the cautious eye of public ſcrutiny, has obtained an eſtabliſhed rank in the art of healing.

LETTER XL.

On the celebrated Problem of the Longitude: general Deſcription of the Earth, of its Axis, its two Poles, and the Equator.

YOU will by this time, no doubt, imagine that enough has been ſaid of electricity; and indeed I have nothing farther to add on that ſubject; and am, of courſe, not a little embarraſſed about the choice of one worthy of your attention.

In order to determine my choice, I think myſelf obliged to take into conſideration thoſe ſubjects which moſt materially intereſt human knowledge, and which Authors of celebrity moſt frequently bring forward. Theſe are ſubjects, reſpecting which, it is to be preſumed, perſons of quality have conſiderable information.

As you muſt, unqueſtionably, have heard frequent mention made of the celebrated problem of the longitude, to the ſolution of which the Britiſh nation has propoſed a moſt magnificent premium, I preſume that my labour will not be wholly thrown away, if I employ it in laying before you a fair ſtate of that important queſtion. It has ſuch an intimate connection with the knowledge of our terraqueous globe, that it were a ſhame to be ignorant of it. It will accordingly furniſh me with an opportunity of explaining a variety of intereſting articles, which, I flatter myſelf, you would wiſh to ſee elucidated.

I begin then with a general deſcription of the earth, which may be conſidered as a globe, though it has been diſcovered by recent obſervation, that its real figure is a ſpheroid, ſomewhat flattened; but the difference is ſo ſmall, that it may, for the preſent, be altogether neglected.

The firſt thing to be remarked on the globe of the earth, are two points on it's ſurface, denominated the two poles of the earth. Round theſe two points the globe of the earth every day revolves, as you turn a ball fixed between the two points of a turning machine. This motion is called the daily or diurnal motion of the earth, each revolution of which is performed in about twenty-four hours. Or, to ſpeak according to appearances, you know that the whole heavens, which we conſider as a concave ball, within whoſe circumference the earth revolves, appear to turn round the earth in the ſame ſpace of twenty-four hours. This motion is likewiſe performed round two fixed points in the heavens, denominated the poles of heaven; now if we conceive a ſtraight line drawn from one of theſe poles of heaven to the other, that line will paſs through the centre of the earth.

But you will eaſily comprehend, that the appearance muſt be the ſame, whether the earth turns round theſe poles, while the heavens remain in a ſtate of reſt; or whether the heavens revolve round their poles, the earth remaining at reſt. On either ſuppoſition we are equally led to the knowledge of the poles of the earth, the foundation not only of aſtronomy, but likewiſe of geography.

Let *fig.* 1. of *plate* I. repreſent the globe of the earth, whoſe poles are the points A and B; one of theſe poles, A, is named the *ſouth* or *antarctic pole*, the other, B, is denominated the *north* or *arctic pole*. This laſt is nearer to the region of the globe which we inhabit.

I remark, that theſe two poles are directly oppoſite to each other; in other words, were a ſtraight line AB to be drawn directly through the earth, it would paſs preciſely through the middle C, that is to ſay, through the centre of the earth. This ſtraight line AB has accordingly it's appropriate name, and is called *the axis of the earth*, which being produced in both directions to the heavens, will terminate in the two points which are called the poles of heaven; and to which we give the ſame names as to thoſe of the earth.

Theſe two poles of the earth are by no means a mere fiction, or a ſpeculation of aſtronomers and geographers; but are really moſt eſſential points marked on the ſurface of our globe; for it is well known, that the nearer we approach theſe two points, the colder and more rugged the face of nature becomes, to ſuch a degree that the regions adjacent to the poles are abſolutely uninhabitable, from the exceſſive cold which prevails there during the winter. No one inſtance, accordingly, has been produced of any traveller, whether by land or water, who has reached either of the poles. It may be affirmed, therefore, that theſe two ſpots of the earth are altogether inacceſſible.

Having thus determined the two poles of the earth A and B, we may conceive the whole globe divided into two hemiſpheres, DBE and DAE, each of which terminates in one of the poles as its ſummit. For this purpoſe we are to ſuppoſe the globe biſected through its centre C, ſo that the ſection ſhall be perpendicular to the axis of the earth; this ſection will mark, on the ſurface, a circle encompaſſing the whole globe, every where equally diſtant from the two poles. This ſurrounding circle is denominated the *equator*. The regions adjacent to it are the hotteſt, and on that account, as the ancients believed, almoſt uninhabitable; but they are now found to be exceedingly populous, though the heat be there almoſt inſupportable.

But as you remove from the equator, on either ſide, toward the poles, the countries become more and more temperate, till, at laſt, on approaching too near the poles, the cold becomes intolerable.

As the equator divides the earth into two hemiſpheres, each bears the name of the pole contained in it; thus the half D B E, which contains the north pole, is denominated the *northern hemiſphere*, and in it is ſituated all Europe, almoſt the whole of Aſia, part of Africa, and the half of America. The other hemiſphere is, from it's pole, denominated the *ſouthern* *hemiſphere*, and contains the greateſt part of Africa, the other half of America, and ſeveral iſles, which geographers attribute to Aſia, as you will recollect to have ſeen in maps of the world.

18th Auguſt, 1761.

LETTER XLI.

Of the Magnitude of the Earth; *of Meridians, and the ſhorteſt Road from Place to Place.*

HAVING diſtinctly fixed the idea of the poles of the earth and of the equator, which you can much more eaſily imagine on a globe, than I can repreſent by a figure, every other neceſſary idea will readily follow from theſe.

I muſt, however, ſubjoin a further elucidation of conſiderable importance. The axis of the earth paſſing from one pole to the other, through the centre of the earth, is a diameter of the globe, and conſequently is double the length of the radius. A radius of the earth, or the diſtance from every point on the ſurface to the centre is computed to be 860 German miles;* the axis of the earth will therefore contain 1720 German miles. And the equator being a circle whoſe centre is likewiſe that of the earth, it will have the ſame radius, namely 860 miles, the diameter of the equator will accordingly be 1720 miles, and it's whole circumference 5400 miles nearly; ſo that if you were to make a tour of the globe, following the tract of the equator, you muſt perform a journey of almoſt 5400 German miles. This will give you ſome idea of the magnitude of the earth.

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* About 3956 miles Engliſh, or 1432 French leagues; the axis of the earth will accordingly be 7912 miles Engliſh, or 2864 French leagues; and the circumference at the equator 23,736 miles Engliſh, about 8600 leagues of France.—*E. E.*

The equator being a circle, it is ſuppoſed to be divided into 360 equal parts, named *degrees:* a degree of the equator contains therefore 15 German miles,* as 15 times 360 make 5400.

Every degree is again ſubdivided into 60 equal parts, called *minutes*, ſo that every minute contains the fourth part of a German mile, or about 6000 feet; a *ſecond* being the ſixtieth part of a minute, will contain 100 feet.

It being impoſſible to repreſent a globe, on paper, any other way than by a circle, you muſt ſupply this defect by imagination. Accordingly AB, *(plate I. fig. 2.)* being the two poles of the earth; B, the north, and A, the ſouth; DMNE will repreſent the equator, or rather that half of it which is turned toward us, the other being concealed on the oppoſite ſide.

The line DMNE repreſents then a ſemi-circle, as well as BDA and BEA; all theſe ſemi-circles having their centres at that of the globe, C. It is poſſible to imagine an infinite number of other ſemi-circles, all of them drawn through the two poles of the earth A and B, and paſſing through every point of the equator, as BMA, BNA: theſe will all be ſimilar to the firſt BDA and BEA; though in the figure their form appears very different. Imagination muſt correct this, and the fact is apparent on a real globe.

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* 69 Engliſh.

All theſe ſemi-circles drawn from one pole to the other, through whatever point of the equator they may paſs, are denominated *meridians:* or rather, a *meridian* is nothing elſe but a ſemi-circle which, on the ſurface of the earth, is drawn from one pole to the other; and you can eaſily comprehend, that, taking any place whatever on the ſurface of the earth, ſay the point L, you can always conceive a meridian BLMA, which, paſſing through the two poles, takes in it's way the point L. This meridian then is named the *meridian of L.* Suppoſing, for example, L to be Berlin, the ſemi-circle BLMA would be the meridian of Berlin; and in like manner reſpecting every other ſpot of the earth.

You can repreſent to yourſelf a globe, on the ſurface of which are deſcribed all the countries of the earth, the continent, as well as the ſea with it's iſlands. This artificial globe, denominated the *terreſtrial* or *terraqueous globe*, you muſt no doubt be acquainted with. As to all meridians, which can poſſibly be drawn upon it, and a great number of which actually are traced, I remark, that each, being a ſemi-circle, is divided by the equator into two equal parts, each of which is the fourth part of a circle; that is an arch of 90 degrees. Accordingly, BM, BL, BN, BE, are fourth parts of a circle as well as AD, AM, AN, AE; each therefore contains 90 degrees: and it may be farther added, that each is perpendicular to the equator, or forms right angles with it.

Again, were a perſon to travel from the point of the equator M, to the pole B, the ſhorteſt road would be to purſue the track of the meridian MLB, which being an arch of 90 degrees, and a degree containing 15 German miles, would contain 1350 miles,* the diſtance to be paſſed in going from the equator to either of the poles.

You will recollect, that the ſhorteſt road from place to place, is the ſtraight line drawn through any two places; here the ſtraight line, drawn from the point M, in the equator, to the pole B, would fall within the earth, a route which it is impoſſible to purſue, for we are ſo attached to the ſurface of the earth, that we cannot remove from it. For this reaſon the queſtion becomes exceedingly different, when it is aſked, What is the ſhorteſt road, leading from one ſpot on the ſurface of a globe to another? This ſhorteſt road is no longer a ſtraight line, but the ſegment of a circle, deſcribed from one point of the ſurface to another, and whoſe centre is preciſely that of the globe itſelf. This is accordingly in perfect harmony with the caſe in queſtion; for to travel from the point M in the equator, to the pole B, the arch of the meridian MLB, which I have repreſented as the ſhorteſt road, is in effect a ſegment of the circle whoſe centre is preciſely that of the earth.

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* About 6200 miles Engliſh, or 2250 French leagues.

In like manner, if we conſider the ſpot L, ſituated in the meridian BLMA, the ſhorteſt road to go thence to the pole B, will be the arch LB; and if we know the number of degrees which this arch contains, allowing 15 German miles to a degree, we ſhall have the length of the road. But if you were diſpoſed to travel from the ſame ſpot to the equator, by the ſhorteſt track, it would be neceſſary to purſue the track of the arch of the meridian LM, the number of degrees contained in which, reckoning 15 German miles to a degree, would give the diſtance.

We muſt be ſatisfied with expreſſing theſe diſtances in degrees, it being ſo eaſy to reduce them to German miles, while other nations employ miles of various length, ſome greater, ſome ſmaller. Taking, then, the city of Berlin for the ſpot L, we find that the arch LM, which leads to the equator, contains 52 degrees and a half; conſequently to travel from Berlin to the equator, the ſhorteſt road is 787 German miles and a half. But if any one were to go from Berlin to the north pole, he muſt follow the direction of the arch BL, which containing 37 degrees and a half, would be 562 German miles and a half. Theſe two diſtances added, give 1350 German miles for the extent of the arch BLM, which is the fourth part of a circle, or 90 degrees, which contain, as we have ſeen, 1350 German miles.

22d Auguſt, 1761.

LETTER

LETTER XLII.

Of Latitude, and its Influence on the Seaſons, and the Length of the Day.

I BEGIN once more with the ſame figure, *(plate I. fig.* 3.) which muſt by this time be abundantly familiar to you. The whole circle repreſents the globe of the earth; the points A and B it's two poles; B the north or arctic, and A the ſouth or antarctic; ſo that the ſtraight line BA drawn within the earth, and paſſing through it's centre C, is the axis of it. Again DME is the equator which divides it into two hemiſpheres, DBE the northern, and DAE the ſouthern.

Let us now take any ſpot whatever, ſay L, and draw it's meridian BLMA, which, being a ſemicircle, paſſes through the point L, and the two poles B and A. This then is the meridian of the place L, divided by the equator at M into two equal parts, making two-fourths of a circle, each of which contains 90 degrees. I remark farther, that the arch LM, of this meridian, gives us the diſtance of the place L from the equator, and that the arch LB expreſſes the diſtance of the ſame place L, from the pole B.

This being laid down, it is of importance to obſerve that the arch LM, or the diſtance of L from the equator, is denominated the *latitude of the place L*; ſo that the latitude of any place on the globe, is nothing

thing elſe but the arch of the meridian of that place, which is intercepted between the equator and the given place: in other words, the latitude of a place, is the diſtance of that place from the equator, expreſſing ſuch diſtance by degrees, the quantity of which we perfectly know, as each degree contains 15 German miles.

You will readily comprehend, that this diſtance muſt be diſtinguiſhed, according as the given place is in the northern, or ſouthern hemiſphere. In the former caſe, that is, if the given place is in the northern hemiſphere, we ſay it has *north latitude*; but if it is in the ſouthern hemiſphere, we ſay it is in *ſouth latitude*.

Taking Berlin as an inſtance, we ſay it is in 52 degrees and 31 minutes of north latitude; the latitude of Magdeburg is, in like manner, northern, 52 degrees and 19 minutes. But the latitude of Batavia in the Eaſt Indies is 6 degrees 15 minutes ſouth; and that of the Cape of Good Hope in Africa, is likewiſe ſouth 34 degrees 15 minutes.

I remark by the way, that for the ſake of abbreviation, inſtead of the word *degree* we affix a ſmall cipher (°) to the numeral characters, and inſtead of the word *minute* a ſmall ſlanting bar (′), and inſtead of *ſecond* two of theſe (″); thus the latitude of the obſervatory at Paris is 48° 50′ 10″, N. that is 48 degrees, 50 minutes and 10 ſeconds North. In Peru there is a place named Vlo, whoſe latitude has been found to be 17° 36′ 15″, S. that is, 17 degrees, 36 minutes and 15 ſeconds South. Hence you will underſtand, that if a place were mentioned whoſe latitude was 0° 0′ 0″; ſuch place would be preciſely under the equator, as its diſtance from the equator is 0, or nothing; and in this caſe it is unneceſſary to affix the letter N or S. But were it poſſible to reach a place whoſe latitude was 90° N. it would be preciſely the north pole of the earth, which is diſtant from the equator the fourth of a circle or 90 degrees. This will give you a clear idea of what is meant by the latitude of a place, and why it is expreſſed by degrees, minutes, and ſeconds.

It is highly important to know the latitude of every place, not only as eſſential to Geography, in the view of aſſigning to each its exact ſituation on geographical charts, but likewiſe becauſe on the latitude depend the ſeaſons of the year, the inequalities of day and night, and conſequently the temperature of the place. As to places ſituated directly under the equator, there is ſcarcely any perceptible variation of the ſeaſons, and through the whole year the days and nights are of the ſame length, namely 12 hours. For this reaſon the equator is likewiſe denominated the equinoctial line: but in proportion as you remove from the equator, the more remarkable is the difference in the ſeaſons of the year, and the more likewiſe the days exceed the nights in ſummer, whereas, reciprocally, the days in winter are as much ſhorter than the nights.

You know that the longeſt days, in theſe northern latitudes, are toward the commencement of our ſummer, about the 21ſt of June; the nights, of conſequence, are then the ſhorteſt; and that toward the beginning of our winter, about the 23d of December, the days are ſhorteſt and the nights longeſt: ſo that every where, the longeſt day is equal to the longeſt night. Now in every place, the duration of the longeſt day depends on the latitude of the place. Here, at Berlin, the longeſt day is 16 hours and 38 minutes, and conſequently the ſhorteſt day in winter is 7 hours 22 minutes. In places nearer the equator, or whoſe latitude is leſs than that of Berlin, which is 52° 32′, the longeſt day in ſummer is leſs than 16 hours 38 minutes, and in winter the ſhorteſt day is more than 7 hours 22 minutes. The contrary of this takes place on removing farther from the equator: at Peterſburg, for example, whoſe latitude is 60 degrees, the longeſt day is 18 hours 30 minutes, and conſequently the night is then only 5 hours 30 minutes: in winter, on the contrary, the longeſt night is 18 hours 30 minutes, and then the day is only 5 hours 30 minutes. Were you to remove ſtill farther from the equator, till you came to a place whoſe latitude was 66° 30′, the longeſt day there would be exactly 24 hours, in other words, the ſun would not ſet at that place, at that ſeaſon; whereas in winter the contrary takes place, the ſun not riſing at all on the 23d of December, that is, the night then laſting 24 hours. Now at places ſtill more remote from the equator, and conſequently nearer the pole, for example, at Warthuys in Swediſh Lapland, this longeſt day laſts for the ſpace of ſeveral days together, during which the ſun abſolutely never ſets;

and the longeſt night, when the ſun never riſes at all, is of the ſame duration.

Were it poſſible to reach the pole itſelf, we ſhould have day for ſix months together, and, during the other ſix, perpetual night. From this you comprehend of what importance it is to know accurately the latitude of every ſpot of the globe.

22d Auguſt, 1761.

LETTER XLIII.

Of Parallels, of the firſt Meridian, and of Longitude.

HAVING informed you, that in order to find the meridian of any given place, ſay L, it is neceſſary to draw on the ſurface of the earth a ſemicircle B L M A, paſſing through the two poles B and A, and through the given place L; I remark, *(plate I. fig.* 4.*)* that there is an infinite number of other places, through which this ſame meridian paſſes, and which are conſequently all ſaid to be ſituated under the ſame meridian, whether in the northern hemiſphere, between B and M, or in the ſouthern, between M and A.

Now all the places, ſituated under the ſame meridian, differ as to latitude, ſome being nearer to, or more remote from, the equator than others. Thus the meridian of Berlin paſſes through the city of Meiſſe, and nearly through the port of Trieſte, as well as many other places of leſs note.

You will likewiſe pleaſe to obſerve that, a great many places may have the ſame latitude, that is, may be equally diſtant from the equator, but all of them ſituated under different meridians. In fact, if L is the city of Berlin, whoſe latitude, or the arch LM contains 52° 31′, it is poſſible under any other meridian BN A, a place I, the latitude of which, or the arch I N, ſhall likewiſe be 52° 31′; ſuch are the points F and G, taken in the meridians BDA, BEA. And as a meridian may be drawn through every point of the equator, in which there ſhall be a place whoſe latitude is the ſame with that of Berlin, or the place L, we ſhall have an infinite number of places, all of the ſame latitude. They will be all ſituated in the circle F L I G, all the points of which being equally diſtant from the equator, it is denominated a *parallel circle* to the equator, or ſimply a *parallel.* A parallel on the globe, then, is nothing elſe but a circle parallel to the equator, that is, all the points of which are equidiſtant from it; hence it is evident, that all the points of a parallel are likewiſe equidiſtant from the poles of the earth.

As it is poſſible to draw ſuch a parallel through every place on the globe, we can conceive an infinite number of them, all differing in reſpect of latitude, each having a latitude, whether north or ſouth, peculiar to itſelf.

You muſt likewiſe be abundantly ſenſible, that the greater the latitude is, or the nearer you approach to either of the poles, the ſmaller the parallels become, till at laſt, on coming to the very poles, where the latitude is 90°, the parallel is reduced to a ſingle point. But, on the contrary, as you approach the equator, or the ſmaller the latitude is, the greater are the parallels; till at laſt, when the latitude becomes 0, or nothing, the parallel is loſt in the equator. It is accordingly by the latitude that we diſtinguiſh them; thus the parallel of 30°, is that which paſſes through every place whoſe latitude is 30 degrees, but it is neceſſary to explain yourſelf, according as you mean north or ſouth latitude.

On conſulting an accurate map, you will obſerve that Hanover is ſituated under the ſame parallel with Berlin, the latitude of both being 52° 31′, and that the cities of Brunſwick and Amſterdam fall nearly under the ſame parallel; but that the meridians paſſing through theſe places are different. If you know the meridian and the parallel under which any place is ſituated, you are enabled to aſcertain its actual poſition on the globe. If it were affirmed, for example, that a certain place is ſituated under the meridian BNA, and the parallel FLG, you would only have to look where the meridian BNA is interſected by the parallel FLG, and the point of interſection I, will give the true poſition of the given place.

Such are the means employed by geographers to determine the real ſituation of every place on the face of the globe. You have only to aſcertain its parallel, or the latitude, and its correſponding meridian. As to the parallel, it is eaſy to mark and diſtinguiſh it from every other; you have only to indicate the latitude, or diſtance from the equator, according as it is north or ſouth: but how deſcribe a meridian, and diſtinguiſh it from every other? They have a perfect reſemblance, they are all equal to each other, and no one has a ſpecial and diſtinctive mark. It depends therefore upon ourſelves to make choice of a certain meridian, and to fix it, in order to refer all others to that one. If, for example, in the figure referred to at the beginning, we were to fix on the meridian BDA, it would be eaſy to indicate every other meridian, ſay BMA, by ſimply aſcertaining on the equator the arch DM, contained between the fixed meridian BDA and the one in queſtion BMA, adding only in what direction you proceed from the fixed meridian toward the other, whether from eaſt to weſt, or weſt to eaſt.

This fixed meridian, to which every other is referred, is called the *firſt meridian;* and the choice of this meridian being arbitrary, you will not think it ſtrange that different nations ſhould have made a different choice. The French have fixed on the iſle of Ferro, one of the Canaries, for this purpoſe, and draw their firſt meridian through it. The Germans and Dutch draw theirs through another of the Canary iſlands, called Teneriffe. But whether you follow the French or German geographers, it is always neceſſary carefully to mark on the equator the point through which the firſt meridian paſſes; from this point you afterwards reckon by degrees, the points through which all other meridians paſs; and both French and Germans have agreed to reckon from weſt to eaſt.

If, therefore, in the figure to which I have already referred, the ſemicircle BDA be the firſt meridian, and the points of the equator M and N were ſituated toward the eaſt, you have only, in order to mark any other meridian, ſay BMA, to indicate the magnitude of the arch DM; and this arch is what we call the *longitude* of all the places ſituated under the meridian BMA. In like manner, all the places ſituated under the meridian BNA have their longitude determined by the arch of the equator DN, expreſſed in degrees, minutes and ſeconds.

29th Auguſt, 1761.

LETTER XLIV.

Choice of the Firſt Meridian.

YOU have now received complete information reſpecting what is denominated the latitude and the longitude of a place on the ſurface of the globe. Latitude is computed on the meridian of the given place, up to the equator; in other words, it is the diſtance of the parallel paſſing through that place, from the equator; and to prevent all ambiguity, it is neceſſary to expreſs whether this latitude or diſtance is north or ſouth.

As to longitude, we muſt determine the diſtance of the meridian of the given place from the firſt meridian; and this diſtance is computed on the equator, from the firſt meridian to the meridian of the given place, always proceeding from weſt to eaſt; in other words, longitude is the diſtance of the meridian of the given place, from the firſt, computing the degrees on the equator, as I have juſt now ſaid.

We always compute, then, from the firſt meridian eaſtward; and it is evident that when we have computed up to 360 degrees, we are brought back preciſely to the firſt meridian, as 360 degrees complete the circumference of the equator. Accordingly, were any particular place found to be in the 359th degree of longitude, the meridian of that place would be only one degree diſtant from the firſt meridian, but toward the weſt. In like manner, 350° of longitude would exactly correſpond with a diſtance of 10° weſtward. For this reaſon, in order to avoid all ambiguity, in determining longitude, we go on to reckon up to 360° toward the eaſt.

You will no doubt have the curioſity to know, why geographers, in ſettling the firſt meridian, have agreed to fix on one of the Canary iſlands? I beg leave to reply, that the intention was to begin with ſettling the limits of Europe toward the weſt; and as theſe iſlands, called the Canaries, and ſituated in the Atlantic ocean, beyond Spain, toward America, were ſtill conſidered as part of Europe, it was thought proper to draw the firſt meridian through the moſt remote of the Canary iſlands, that we might be enabled to compute the other meridians without interruption, not only all over Europe, but through the whole extent of Aſia: from whence, going on to reckon toward the eaſt, we arrive at America, and thence return at length to the firſt meridian.

But to which of the Canary iſles ſhall we give the preference? Certain geographers of France made choice of the iſle of Ferro, and the Germans that of

Teneriffe, becauſe the real ſituation of theſe iſles was not then ſufficiently aſcertained, and it was not perhaps known which of them was the moſt remote; beſides, the German geographers imagined that the mountain named the Peak of Teneriffe was pointed out, as it were, by the hand of Nature for the firſt meridian.

Be this as it may, it ſeems rather ridiculous to draw the firſt meridian through a place whoſe real poſition on the globe is not perfectly determined; for it was not till very lately that the ſituation of the Canaries was aſcertained. For this reaſon, the moſt accurate aſtronomers fix the firſt meridian preciſely 20 degrees diſtant from that of the obſervatory at Paris, without regarding through what ſpot the firſt may in that caſe paſs; and it is undoubtedly the ſureſt method that can be adopted: and in order to determine every other meridian, the ſimpleſt way is to find out its diſtance from that of Paris: then if that other meridian is more to the eaſt, you have only to add to it 20 degrees, in order to have the longitude of the places ſituated under it: but if this meridian be weſtward to that of Paris, you muſt ſubtract the diſtance from 20 degrees: finally, if this diſtance toward the weſt is more than 20 degrees, you ſubtract it from 380 degrees, that is, from 20 degrees above 360, in order to have the longitude of the meridian.

Thus the meridian of Berlin being to the eaſtward of the meridian of Paris 11° 7′ 15″, the longitude of Berlin will be 31° 7′ 25″; and this is likewiſe the longitude of all other places ſituated under the ſame meridian with Berlin.

In like manner the meridian of Peterſburg being 28 degrees more to the eaſt than that of Paris, the longitude of Peterſburg will be 48°.

The meridian of St. James's, London, is more to the weſt than that of Paris by 2° 25′ 15″; ſubtracting, therefore, that quantity from 20°, the remainder, 17° 34′ 45″, gives the latitude of St. James's, London.

Let us now take the city of Lima in Peru, the meridian of which is 70° 9′ 30″ to the weſtward of that of Paris; that diſtance muſt be ſubtracted from 380 degrees; which will leave a remainder of 309° 50′ 30″, the longitude of Lima.

Now, when the latitude and longitude of a place are known, we are enabled to aſcertain its true poſition on the terreſtrial globe, or on a map: for as the latitude marks the parallel, under which the place is ſituated, and the meridian gives the meridian of the ſame place, the point where the parallel interſects the meridian, will be exactly the place in queſtion.

You have but to look at a map, that of Europe for example; and you will ſee the degrees of the parallels marked on both ſides, or their diſtances from the equator: above and below are the degrees of longitude, or the diſtances of the ſeveral meridians from the firſt.

The parallels and meridians are uſually traced on maps degree by degree, ſometimes at the diſtance of 5 degrees from each other. In moſt maps the meridians are drawn up and down, and the parallels from left to right: the upper part is directed toward the north; the under to the ſouth: the right-hand ſide toward the eaſt, and the left-hand ſide toward the weſt.

It is likewiſe to be remarked, that as all the meridians meet at the two poles, the more any two meridians approach to either of the poles, the ſmaller their diſtance becomes; at the equator their diſtance always is greateſt. Accordingly on all good maps, where the meridians are traced, you will obſerve that they gradually approximate toward the top, that is the north, and their diſtances increaſe as you proceed toward the equator. This is all that ſeems to be requiſite for the underſtanding of geographical charts, by means of which an attempt is made to repreſent the ſurface, or part of the ſurface, of the globe.

But my principal object was to demonſtrate how the real poſition of every ſpot on the globe is determined by its latitude and longitude.

1ſt September, 1761.

LETTER XLV.

Method of determining the Latitude, or the Elevation of the Pole.

IT being a matter of ſuch importance to know the latitude and longitude of every place, in order to aſcertain exactly the ſpot of the globe where you are, you muſt be ſenſible that it is equally important to diſcover the means of certainly arriving at ſuch knowledge.

Nothing can be more intereſting to a man, who has been long at ſea, or after a tedious journey through unknown regions, than to be informed at what preciſe ſpot he is arrived; whether or not he is near ſome known country, and what courſe he ought to purſue in order to reach it. The only means of relieving ſuch a perſon from his anxiety would undoubtedly be to give him the latitude and longitude of the place where he is: but what muſt he do to attain this moſt important information? Let us ſuppoſe him on the ocean, or in a vaſt deſert, where there is no one whom he could conſult. After having aſcertained, by the help of a terreſtrial globe, or of maps, the latitude and longitude of the place where he is, he will with eaſe, from them, determine his preſent poſition, and be furniſhed with the neceſſary information reſpecting his future progreſs.

I proceed, therefore, to inform you, that it is by aſtronomy chiefly we are enabled to determine the latitude and longitude of the place where we are; and that I may not tire you by a tedious detail of all the methods which aſtronomers have employed for this important purpoſe, I ſhall ſatisfy myſelf with preſenting a general idea of them, flattering myſelf that this will be ſufficient to convey to you the knowledge of the principles on which every method is founded.

I begin with the latitude, which is involved in ſcarcely any difficulty, whereas the determination of the longitude ſeems hitherto to have defied all human reſearch, eſpecially at ſea, where the utmoſt preciſion is requiſite. For the diſcovery of this laſt, accordingly, very conſiderable prizes have been propoſed, as an encouragement to the learned to direct their talents and their induſtry toward a diſcovery ſo intereſting, both from its own importance, and from the honour and emolument which are to be the fruit of it.

I return to the latitude, and the means of aſcertaining it, referring to ſome future opportunity a more ample diſcuſſion of the longitude, and of the different methods of diſcovering it, eſpecially at ſea.

Let the points B and A *(plate I. fig. 5.)* be the poles of the earth; BA its axis, and C its centre; let the ſemicircle BDA repreſent a meridian, interſected by the equator at the point D; and BD, AD, will be each the quadrant of a circle, or an arch of 90 degrees: the ſtraight line DC will therefore be a radius of the equator, and DE its diameter.

Let there now be aſſumed in this meridian BDA, the point L, the given place, of which the latitude is required, or in other words, the number of degrees contained in the arch LD, which meaſures the diſtance of the point L from the equator; or again, drawing the radius CL, as the arch LD meaſures the angle DCL, which I ſhall call *y*, this angle *y* will expreſs the latitude of the place L, which we want to find.

Now it being impoſſible to place ourſelves at the centre pole is 52° 31′, and hence we conclude that the latitude of that city is likewiſe 52° 31′.

This is one very remarkable inſtance to demonſtrate how the heavens may aſſiſt us in the attainment of the knowledge of objects which relate only to the earth.

5th September, 1761.

LETTER XLVI.

Knowledge of the Longitude, from a Calculation of the Direction, and of the Space paſſed through.

I NOW proceed to the longitude; and remark that, on taking a departure, whether by land or water, from a known place, it would be eaſy to aſcertain the ſpot we had reached, did we know exactly the length of the road, and the direction which we purſued. This might, in ſuch a caſe, be effected even without the aid of aſtronomy; and this obliges me to enter into a more particular detail on the ſubject.

We meaſure the length of a road by feet; we know how many feet go to a mile, and how many miles go to an arch of one degree upon the globe: thus we are enabled to expreſs in degrees the diſtance we have travelled.

As to the route or direction in which we travel, it is neceſſary accurately to know the poſition of the meridian at every place where we are. As the meridian proceeds in one direction toward the north pole, and in the other toward the ſouth; you have only to draw, on the horizon of the ſpot where you are, a ſtraight line from north to ſouth, which is called the *meridian line* of that place. All poſſible care muſt be taken to trace this meridian line very accurately, and here the heavens muſt again perform the office of a guide.

You know it is mid-day when the ſun is at his greateſt elevation above the horizon; or, which is the ſame thing, the direction of the ſun is then exactly ſouth, and the ſhadow of a ſtaff fixed perpendicularly on a horizontal plane will fall, at that inſtant, preciſely northward. Hence it is eaſy to comprehend, how an obſervation of the ſun may furniſh us with the means of accurately tracing a meridian line, wherever we may be.

Having traced a meridian, every other direction is very eaſily determined.

Let the ſtraight line N S *(plate I. fig. 6.)* be the meridian, one of the extremities N being directed toward the north, and the other S toward the ſouth. With this meridian let there be drawn at right angles the ſtraight line E W, whoſe extremity E ſhall be directed toward the eaſt, and the other extremity W toward the weſt. Having divided the circle into 16 equal parts, we ſhall have ſo many different directions, denominated according to the letters affixed to them; and in caſe of not purſuing a direction which exactly correſponds with ſome one of the ſixteen, the angle muſt be marked which that deviating line of direction makes with the meridian NS, or with EW, which is perpendicular to it.

It is thus we are enabled to determine exactly the direction which we purſue in travelling; and ſo long as we are aſſured of the length of the way, and of the direction purſued, it will be very eaſy to aſcertain the true place at which we have arrived, and to indicate both its longitude and latitude. We employ, for this purpoſe, an accurate map, which contains the point of departure, and that which we have reached; and by means of the ſcale, which gives the quantity of miles or leagues that go to a degree, it is eaſy to trace, on ſuch map, the track purſued and completed.

Figure 7. of plate I. repreſents a map, on which are marked from left to right the degrees of latitude, and thoſe of longitude from top to bottom; it is likewiſe viſible, on the face of it, that the meridians converge as they approach toward the north, and retire from each other toward the ſouth, as is the actual caſe on the globe.*

This map contains part of the ſurface of the earth, from the 53d degree of north latitude to the 59th degree; and from the 13th degree of longitude to the 26th.

Suppoſe, then, I take my departure from the place

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* All that the author ſays in the ſequel on this ſubject, is applicable, ſtrictly ſpeaking, only to marine charts; but apparently he did not think himſelf obliged to enter into any detail on their particular conſtruction.—*F. E.*

L, the longitude of which is 16°, and the latitude 57° 20′, and that I proceed in the direction ESE; and have travelled a ſpace of 75 German miles. In order to determine the longitude and latitude of the place I have reached, I draw from the place L the ſtraight line LM, making with the meridian 16° 16′ the ſame angle, which the direction ESE in the preceding figure makes with N. Then on that line I take, according to the ſcale marked on the chart, LM equal to 75 German miles, and the point M ſhall be the place which I have reached.

I have then only to compare this place with the meridians and parallels traced on the map, and I find that it's longitude is 24° nearly; and on meaſuring more exactly the part of the degree to be added to the 24th degree, I find the longitude of the point M to be 24° 4′. As to the latitude, I obſerve it to be between the 55th and 56th degree, and, by an eaſy computation, I find it to be 55° 25′; ſo that the latitude of the place M, which I have reached, is 55° 25′, and its longitude 24° 4′.

It has here been ſuppoſed that I have invariably purſued the ſame direction ESE, from firſt to laſt: but if I have from time to time deviated from that direction, I have only to perform the ſame operation on each deviation, to find the place where I then was; from this I take a freſh departure, and trace my direction till another deviation takes place, and ſo on, till I reach my object. By theſe means it is always in my power, whether travelling by ſea or land, to aſcertain the place I have reached; provided when I know exactly, through my whole progreſs, the direction I purſue, and meaſure with equal accuracy the length of the way.

We might in this caſe diſpenſe even with the aſſiſtance of aſtronomy, unleſs we had occaſion for it accurately to determine our direction, or the angle which it makes with the meridian; but the magnetic needle or compaſs may, in many caſes, ſupply this want.

You muſt be ſenſible, however, that it is poſſible to make a very conſiderable miſtake both in the computation of the direction, and of the length of the way, eſpecially in very long voyages. How often is it neceſſary to change the direction in travelling even from hence to Magdeburg? and how is it poſſible to meaſure exactly the length of the way? But when we travel by land, we are not reduced to this expedient: for we are enabled to meaſure by geometrical experiments the diſtance of places, and the angles which the diſtances make with the meridian of every place; and thus we can determine, with tolerable accuracy, the true ſituation of all places.

8th September, 1761.

LETTER XLVII.

Continuation. Defects of this Method.

A METHOD of obſerving the direction purſued, and the length of the courſe, ſeems to be of ſingular utility in ſea voyages, becauſe there we are not under the neceſſity of deviating from the direction every moment, as in travelling by land; for, with the ſame wind, we can proceed in the ſame direction.

Pilots are accordingly very attentive in exactly obſerving the courſe of the veſſel, and in meaſuring the progreſs ſhe has made. They keep an accurate journal of all theſe obſervations, at the cloſe of every day, nay ſtill more frequently; they trace on their ſea-charts the progreſs they have made, and thus are enabled to mark on the charts, for every period of time, the point where they are, and of which they conſequently know the latitude and longitude. Accordingly, ſo long as the courſe is regular, and the veſſel is not agitated by a tempeſt, good pilots are ſeldom miſtaken: but when they are in doubt, they have recourſe to aſtronomical obſervations, from which they diſcover the elevation of the pole; and this being always equal to the latitude of the place where they are, they compare it with that which they have marked on the chart, conformably to the computation of their progreſs. If theſe are found to coincide, their computation is juſt; if they diſcover a difference, they conclude with certainty that ſome error has been committed, in the computation of the diſtance, and of the courſe; in that caſe, they re-examine both the one and the other more carefully, and endeavour to apply the neceſſary corrections, in order to make the computation agree with the obſervation of the height of the pole, or of the latitude, which is equal to it.

This precaution may be ſufficient in ſhort voyages, as the errors committed can in theſe be of no great importance; but in very long voyages, theſe ſlight miſtakes may accumulate to ſuch a degree, that, at laſt, a very groſs miſtake may be committed, and the place where the veſſel actually is may differ conſiderably from what it was ſuppoſed to be on the chart.

I have hitherto gone on the ſuppoſition that the voyage proceeded quietly; but ſhould a ſtorm ariſe, during which the veſſel is ſubjected to the rudeſt concuſſions of wind and waves, it is evident that the computation of diſtance and courſe is entirely deranged, and that it is impoſſible to trace on the chart the progreſs ſhe has made.

It would be very eaſy, after this derangement, to aſcertain, by aſtronomical obſervations, the latitude of the ſhip's place; but this would determine only the parallel of that place, and it would remain totally uncertain at what point of the parallel ſhe actually was.

It is neceſſary, therefore, to diſcover likewiſe the longitude of the place, which ſhews us the meridian under which it is ſituated; and then the interſection of that meridian with the parallel found, will give the veſſel's true place. This will make you ſenſible of what importance it is to aſſiſt mariners in diſcovering likewiſe the longitude of the place where they are.

This neceſſity is impoſed not only from the conſideration of the tempeſts to which navigation is liable; for it is poſſible, ſuppoſing the voyage to proceed ever ſo quietly, to be groſsly miſtaken in the computation of both courſe and diſtance. Could we ſuppoſe the ſea to be at reſt, it might be poſſible to invent various methods of aſcertaining, with tolerable exactneſs, the way which the veſſel has made; but there are rapid currents in many places of the ocean, which have the reſemblance of a river running in a certain direction. Thus it is obſerved, that the Atlantic ocean has a perpetual current into the Mediterranean ſea, through the ſtraits of Gibraltar; and that the ocean, between Africa and America, has a very conſiderable current from eaſt to weſt, ſo that a voyage to America is performed in much leſs time than a voyage from America to Europe.

Were ſuch currents conſtant and well known, we ſhould have conſiderable aſſiſtance toward forming our calculations: but it has been obſerved, that they are ſometimes more, ſometimes leſs rapid, and that they frequently change their direction; which deranges the calculations of the moſt ſkilful navigator to ſuch a degree, that it is no longer ſafe to truſt them. We have but too many fatal inſtances of ſhips daſhed on concealed rocks, and loſt, becauſe theſe were computed to be ſtill at a conſiderable diſtance. It was afterwards diſcovered, when too late, that theſe calamities had been occaſioned by the currents of the ocean, which deranged the calculations of navigators.

In fact, when the ocean has a current which makes it flow like a river, following a certain direction, veſſels caught in it are carried away imperceptibly. In a river we clearly perceive that the current is carrying us along, by obſerving the banks or the bottom: but at ſea no land is viſible, and the depth is too great to admit of our making any obſervation from the bottom. At ſea, then, it is impoſſible to diſcern the currents; and hence ſo many dreadful miſtakes, reſpecting both courſe and diſtance. Whether, therefore, we take tempeſts into the account, or not, we are always under the neceſſity of falling on other methods of aſcertaining the longitude of the places where we may arrive; and of the various methods hitherto employed for acquiring this knowledge of the longitude, I now proceed to inform you.

12th September, 1761.

LETTER XLVIII.

Second Method of determining the Longitude, by Means of an exact Time Piece.

A VERY ſure method of finding the longitude, would be a clock, watch, or pendulum, ſo perfect, that is to ſay, which ſhould always go ſo equally, and ſo exactly, that no concuſſion ſhall be able to affect its motion.

Suppoſing ſuch a time-piece conſtructed, let us ſee in what manner, by means of it, we ſhould be enabled to ſolve the problem of the longitude. We

muſt

Fig. 1

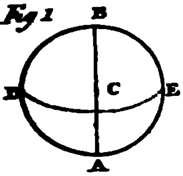


Fig. 2.

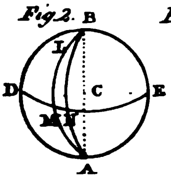


Fig. 3.

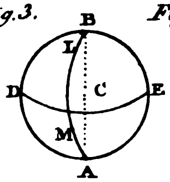


Fig. 4.

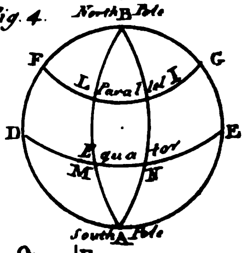


Fig. 6.

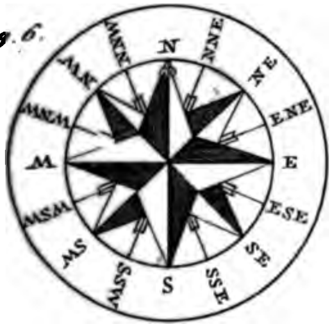


Fig. 5

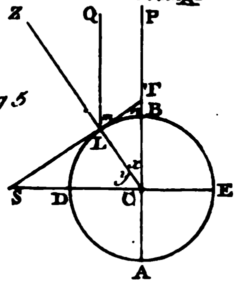


Fig. 7

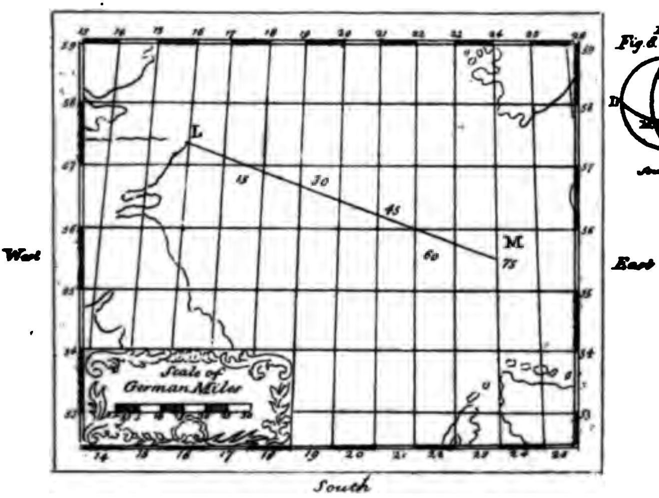
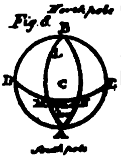


Fig. 8.



muſt return, for this purpoſe, to the conſideration of meridians, which we are to conceive to be drawn through every place on the ſurface of the globe.

You know that the ſun ſeems to deſcribe every day a circle round the earth, and that, of conſequence, he paſſes ſucceſſively over all the meridians in the ſpace of twenty-four hours.

Now, the ſun is ſaid to paſs *over*, or *through* a given meridian, if a ſtraight line drawn from the ſun to the centre of the earth C, (*plate I. fig.* 8.) paſs preciſely through that meridian. If therefore, in the preſent caſe, the line drawn from the ſun to the centre of the earth paſs through the meridian BLMA, we would ſay that the ſun was in that meridian, and then it would be mid-day to all the places ſituated under ſuch meridian; but under every other, it would not be mid-day at that preciſe inſtant; it would there be before noon or after it, every where elſe.

If the meridian BNA is ſituated to the eaſtward of the meridian BMA, the ſun, in making his circuit from eaſt to weſt, muſt paſs over the meridian BNA before he reaches the meridian BMA; conſequently it will be mid-day under the meridian BNA earlier than under the meridian BMA; when, therefore, it ſhall be mid-day under this laſt meridian, mid-day under every other meridian to the eaſtward will be already paſt, or it will be afternoon with them. On the contrary, it will be ſtill forenoon under every meridian, ſay BDA, ſituated to the weſtward, as the ſun cannot reach it till he has paſſed over the meridian BMA.

And as the motion of the ſun is regular and uniform, and he completes his circuit of the globe, that is 360 degrees, in twenty-four hours, he muſt every hour deſcribe an arch of 15 degrees. When, therefore, it is noon at Berlin, and at every other place ſituated under the ſame meridian, noon will be already paſt under meridians ſituated to the eaſtward; and more particularly ſtill under the meridian ſituated 15 degrees to the eaſtward of that of Berlin, it will already be one o'clock; under the meridian 30 degrees eaſtward, two o'clock; under that of 45 degrees, three o'clock afternoon, and ſo on. The contrary will take place under meridians ſituated to the weſtward of that of Berlin; when it is noon there, it will be only eleven o'clock forenoon under the meridian 15 degrees to the weſtward, ten o'clock under the meridian of 30, nine o'clock under the meridian of 45 degrees weſtward, and ſo on; a difference of 15 degrees between two meridians always amounting to an hour of time.

To elucidate ſtill more clearly what has now been remarked, let us compare the two cities Berlin and Paris. As the meridian of Berlin is 11° 17′ 15″, to the eaſtward of that of Paris, reckoning an hour to 15 degrees, this difference of 11° 7′ 15″, will give 44 minutes and 29 ſeconds of time, or three quarters of an hour nearly. When, therefore, it is midday at Paris, it will be 44 minutes and 29 ſeconds after mid-day at Berlin; and reciprocally, when it is mid-day at Berlin, it will only be 15 minutes and 31 ſeconds after eleven o'clock at Paris: ſo that it will not be noon at this laſt city till 44 minutes and 29 ſeconds afterwards. Hence it is evident that the clocks at Berlin ſhould always be faſter than thoſe of Paris, and that this difference ought to be 44 minutes and 29 ſeconds.

The difference between the meridians of Berlin and Magdeburg is 1° 40′, Berlin therefore is to the eaſtward of Magdeburg; and this difference reduced to time gives 6 minutes and 40 ſeconds, which the clocks of Berlin ought to indicate more than that of Magdeburg. Conſequently, if it is juſt now noon at Magdeburg, and the clocks there, which I ſuppoſe well regulated, point to XII. the clocks at Berlin ſhould, at the ſame inſtant, indicate 6 minutes and 40 ſeconds after XII, that is, noon there is already paſt.

Hence you ſee, that in proportion as places differ in longitude, or as they are ſituated under different meridians, well regulated time-pieces ought not to point out the ſame hour at the ſame inſtant, but the difference ought to be a whole hour, when that of the longitude is 15 degrees.

In employing a time-piece then for aſcertaining the longitude of the places through which we paſs, it would firſt be neceſſary to regulate it exactly at ſome place where we actually were. This is done by obſerving the inſtant of noon, that is the inſtant when the ſun paſſes over the meridian of that place; and the time-piece ought then to point preciſely to XII. It ought afterwards to be adjuſted in ſuch a manner, that always after a revolution of 24 hours, when the ſun returns to the meridian, the index, after having made two complete circuits, ſhould again point exactly to XII. If this is carefully obſerved, ſuch well regulated time-pieces will not coincide in different places, unleſs theſe be ſituated under one and the ſame meridian; but if they are ſituated under different meridians, that is if there be a difference of longitude, the time indicated by the clock or watch, at the ſame moment, will likewiſe be different; at the rate of one whole hour of time for every 15 degrees of longitude.

Knowing then the difference of time, indicated by well regulated time-pieces, at different places, and at the ſame inſtant, we are enabled exactly to compute the difference of longitude at theſe two places, reckoning always 15 degrees for an hour, and the fourth part of a degree for a minute.

15th September, 1761.

LETTER XLIX.

Continuation, and farther Elucidations.

YOU will be leſs ſurpriſed at the difference of time which well regulated time-pieces muſt indicate, under different meridians, when you reflect, that while it is noon with us, there are countries toward the eaſt, where the ſun is already ſet, and that there are others toward the weſt, where he is but juſt riſing. It muſt therefore be already night with the one, and ſtill morning with the other, at the ſame inſtant that it is noon with us. You know, beſides, that with our antipodes, who are under the meridian diametrically oppoſite to ours, it is night, while it is day with us; ſo that our noon correſponds exactly to their midnight.

It will be an eaſy matter, after theſe elucidations, to ſhew how an exact time-piece may aſſiſt us in diſcovering the difference of meridians, or that of the longitude, at different places.

Suppoſing me poſſeſſed of ſuch an excellent time-piece, which, once exactly regulated, ſhews me every day the preciſe time* it is at Berlin, ſo that whenever it is noon at Berlin, it points preciſely to XII.: ſuppoſing farther, that it goes ſo regularly, that once adjuſted, I have no farther occaſion to touch it, and that it's motion is not to be deranged either by the ſhaking of a carriage, or the agitation of a veſſel on the ocean, or by any concuſſion whatever to which it may be expoſed.

Provided thus, with a time-piece of this deſcription, I ſet out to travel, whether by land or by ſea; perfectly aſſured that, go where I will, it's motion will be ſteady and uniform, as if I had remained at Berlin: it will every day point to XII. at the very moment it is noon at Berlin, and that, wherever I may happen to be. On this journey, I arrive firſt at Magdeburg: there I obſerve the ſun when he paſſes the meridian, and this happens when he is exactly ſouth; and it being then noon at Magdeburg, I conſult my time-piece, and obſerve it points to 6 minutes and 40 ſeconds after XII.: whence I conclude, that when it is noon at Magdeburg, noon at Berlin is already paſt, and that the difference is 6′ 40″, of time, which correſpond to 1° 40′ of diſtance; therefore the meridian of Magdeburg is to the weſtward of that of Berlin. The longitude of Berlin, therefore, being 31° 7′ 15″, the longitude of Magdeburg will be 1° 40′ leſs, that is, it will be 29° 27′ 15″.

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* We muſt here underſtand the *mean* (in French *moyen*) time whoſe relation to the true time is laid down in aſtronomical tables. The author deemed it unneceſſary here to point out that diſtinction, as it would have led to a detail too minute.—*F. E.*

I thence proceed to Hamburgh, accompanied by my time-piece, which I never touch; and there obſerving when it is noon by the ſun, for I cannot depend on the public clocks which there announce the hour, I find my time-piece already announces 13′ 33″ after XII.; ſo that at Berlin noon is paſſed 13′ 33″ when it is exactly noon at Hamburgh; hence I conclude, that the meridian of Hamburgh is 3° 23′ 15″ to the weſtward of that of Berlin; reckoning 15°, to an hour, that is one degree for every 4 minutes of time; accordingly I find that 13′ 33″ of time give 3° 23′ 15″ of diſtance, for the difference of the meridians. The longitude of Hamburgh will be, of courſe, 27° 44′.

At Hamburgh I go to ſea, ſtill accompanied by my time-piece, and after a long voyage I arrive at a place where, waiting for noon, the moment of which I aſcertain by obſerving the ſun, I find that my time-piece indicates only 58′ 15″ after X. ſo that then it is not yet noon at Berlin, and the difference of time is 1 hour 1′ 45″, from which I conclude, that the place at which I have arrived is to the eaſtward of Berlin; and as one hour gives 15 degrees, one minute of time 15′, and 45 ſeconds of time 11′ 15″, the difference of the meridians will therefore be 15° 26′, 15″. I find then that I am at a place to the eaſtward of Berlin, whoſe longitude is greater than that of Berlin by 15° 26′ 15″; now the longitude of that city being 31° 7′ 15″, the longitude of the place where I am muſt be 46° 33′ 30″. Thus I have diſcovered under what meridian I now am, but I am ſtill uncertain as to the point of the meridian. In order to aſcertain this I have recourſe to aſtronomical obſervations, and find the height of the pole to be preciſely 41°. Knowing likewiſe that I am ſtill in the northern hemiſphere, as I have not paſſed the equator, I diſcover that I actually am at a place whoſe latitude is 41° north, and the longitude 46° 33′ 30″. I take therefore my globe, or maps, and trace the meridian whoſe longitude is 46° 33′ 30″; I look for the place whoſe latitude is 41°, and at the point of interſection I find I have got to the city of Conſtantinople, without having occaſion to apply for information to any perſon whatever.

Thus, at whatever place of the globe I may arrive, poſſeſſed of a time-piece ſo exact, I am able to aſcertain the longitude of it, and then an obſervation of the height of the pole will ſhew me it's latitude. All that remains therefore is to take the terreſtrial globe, or a good map, and it will be eaſy for me to aſcertain where I am, however unknown to me the country may in other reſpects be.

It is much to be regretted, that artiſts of the greateſt ability have hitherto been unſucceſsful in the conſtruction of time-pieces ſuch as I have deſcribed, and ſuch as the caſe requires. We meet with a great many very good pendulum machines, but they go regularly only when fixed in undiſturbed ſituations; the ſlighteſt concuſſion is apt to derange their motion; they are therefore totally uſeleſs in long ſea voyages. It is obvious that the pendulum, which regulates the motion, is incapable of reſiſting the ſhocks to which it is expoſed in navigation. About ten years ago, however, an Engliſh artiſt pretended that he had conſtructed a time-piece proof againſt the motion of a ſhip at ſea, and that after having tried it a long time together in a carriage on the road, it was impoſſible to perceive the ſlighteſt derangement: on which the inventor claimed, and received part of the parliamentary prize propoſed for the diſcovery of the longitude, and the reſt was to be paid, after it had been put to the proof of a long voyage. But ſince that time we have heard no more of it; from which it is to be preſumed that this attempt too has failed, like many others which had the ſame object in view*.

19th September, 1761.

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* The Author is here undoubtedly alluding to Mr. *Harriſon*, whoſe marine time-piece has been tried with ſucceſs in England. Meſſrs. *le Roy* and *Berthout* have, in France, approved their talents in the ſame way.—*E. E.*

LETTER

LETTER L.

Eclipſes of the Moon, a Third Method of finding the Longitude.

FROM want of the exquiſite time-piece, of which I have endeavoured to give you an idea, the eclipſes of the moon have hitherto been conſidered as the moſt certain method of diſcovering the longitude; but theſe phenomena preſent themſelves ſo rarely, that we have it not in our power to employ them ſo often as occaſion requires.

You know that the moon is eclipſed, when it paſſes into the ſhadow of the earth: it is poſſible, then, to obſerve the moment when the moon begins to enter into the ſhade, and when ſhe has emerged; the one is denominated the beginning of the eclipſe, and the other its end; and when both are obſerved, the mean time betwixt them is denominated the middle of the eclipſe. The moon is ſometimes wholly immerged in the ſhadow of the earth, and remains for ſome time inviſible; this we call a total eclipſe, during which we may remark the moment when the moon entirely diſappears, and that when ſhe begins to emerge; the former is called the beginning of total darkneſs, and the latter, the end of it. But when a part only of the moon is obſcured, we call it a partial eclipſe, and we can remark only the moment of its beginning and ending. You know likewiſe that eclipſes of the moon can happen only at the full, and that but rarely.

When, therefore, an eclipſe of the moon is obſerved at two different places ſituated under different meridians, the beginning of the eclipſe will be clearly ſeen at both, and at the ſame inſtant, but the time-pieces at theſe different places will by no means indicate the ſame hour, or any other diviſion of time exactly the ſame: I mean well regulated time-pieces, each of which points preciſely to XII. when it is noon at that place. If theſe places are ſituated under the ſame meridian, their time-pieces will no doubt indicate the ſame time at the beginning and at the end of the eclipſe. But if theſe two meridians are 15 degrees diſtant from each other, that is, if the difference of their longitude be 15°, the time-pieces muſt differ a complete hour, from the beginning to the end of the eclipſe; the time-piece of the place ſituated to the eaſtward will indicate one hour more than the other; the difference of 30° in longitude will occaſion that of two hours in the time indicated by well regulated clocks or watches; and ſo on, according to the following table.

DIFFERENCE OF LONGITUDE.	
of Degrees.	of Time.
15°.	1 hour.
30°.	2.
45°.	3.
60°.	4.
75°.	5.
90°.	6.
105°.	7.
120°.	8.
135°.	9.
150°.	10.
165°.	11.
180°.	12.

If therefore the difference of longitude were 150°, the time-pieces would differ 10 hours from the beginning to the end of the eclipſe.

Thus when the ſame eclipſe is obſerved at two different places, and the moment of it's commencement is exactly marked on the time-pieces at each, it will be eaſy to calculate, from the difference of the time indicated, the difference of longitude between the two places. Now, that where the time is more advanced, muſt be ſituated more toward the eaſt, and conſequently it's longitude greater, as longitude is reckoned from weſt to eaſt.

By ſuch means, accordingly, the longitude of the principal places on the globe have been determined, and geographical charts are conſtructed conformably to theſe determinations. But it is always neceſſary to compare the obſervations made, in a place the longitude of which was not already known, with thoſe which had been made, in a known place, and to wait the reſult of that compariſon. Were I to arrive then, after a long voyage, at an unknown place, and an opportunity preſented itſelf of there obſerving an eclipſe of the moon; this wo l, in the firſt inſtance, afford me no aſſiſtance towa the diſcovery of the longitude of that place; I co l not till after my return compare my obſervation with another made in a known place, and thus I d learn too late where I was at that time. The gra d point in requeſt is, How am I, at the moment, to acquire the neceſſary information, that I may take my meaſures accordingly?

Now the motion of the moon being ſo exactly known, it is poſſible to attain this ſatisfaction, for we are thereby enabled not only to calculate before-hand all future eclipſes, but to aſcertain the moment of the beginning and end, according to the time-pieces of a given place. You know that our Berlin almanacks always indicate the beginning and the end of every eclipſe viſible at that city. In the view, then, of undertaking a long voyage, I can furniſh myſelf with a Berlin almanack, and if an opportunity preſents itſelf of obſerving an eclipſe of the moon at an unknown place, I muſt mark exactly the time of it, by a time-piece accurately regulated by the ſun at noon, and compare the moments of the beginning and end of the eclipſe with thoſe indicated in the almanack, in order to aſcertain the difference between the meridian of Berlin, and that which paſſes through the place where I am.

But beſide the rarity of eclipſes of the moon, this method is ſubject to a farther inconvenience; we are not always able to diſtinguiſh, with ſufficient accuracy, the moment of the beginning and end of the eclipſe, which comes on ſo imperceptibly that a miſtake of ſeveral ſeconds may very eaſily be committed. But as the miſtake will be nearly the ſame at the end as at the beginning, we calculate the middle point of time between the two moments obſerved, which will be that of the eclipſe, and we afterward compare this with that which is indicated by the almanack for Berlin, or for any other known place.

If the almanack for next year ſhould not be publiſhed, when I ſet out on my voyage, or ſuppoſing it to laſt more years than one, there are books containing the eclipſes calculated for ſeveral years to come.

22d September, 1761.

LETTER LI.

Obſervation of the Eclipſes of the Satellites of Jupiter, a Fourth Method of finding the Longitude.

ECLIPSES of the ſun may likewiſe aſſiſt in aſcertaining the longitude, but in a way that requires more profound reſearch, becauſe the ſun is not immediately obſcured; it is only the interpoſi-

tion

tion of the body of the moon, which obſtructs the tranſmiſſion of his rays to us; as when we employ a paraſol to ſhelter us from them, which prevents not others from beholding all their luſtre. For the moon conceals the ſun only from part of the inhabitants of the earth; and an eclipſe of the ſun may be clearly viſible at Berlin, while at Paris there is no interception of his light.

But the moon is really eclipſed by the ſhadow of the earth; her own light is diminiſhed or extinguiſhed by it; hence the eclipſes of the moon are ſeen in the ſame manner, wherever ſhe is above the horizon at the time of the eclipſe.

It cannot have eſcaped your penetration, that if there were other heavenly bodies which, from time to time, underwent any real obſcuration, they might be employed with ſimilar ſucceſs, as the eclipſes of the moon, in aſcertaining the longitude. The ſatellites of Jupiter, which paſs ſo frequently into the ſhadow of their planet, that almoſt every night one or other of them is eclipſed, may be ranked in the number of theſe, and furniſh us with another excellent method of determining the longitude. Aſtronomers accordingly employ it with great ſucceſs.

You know that Jupiter has four ſatellites, which make their revolutions round him, each in his own orbit, as repreſented in the annexed figure (*plate II. fig.* 1.) by circles deſcribed round Jupiter. I have likewiſe repreſented the ſun in this figure, in order to exhibit the ſhadow AOB behind the body of Jupiter. You ſee the firſt of theſe ſatellites, marked 1, on the point of entering into the ſhadow; the ſecond, marked 2, has juſt left it; the third, 3, is ſtill at a great diſtance, but approaching to it; and the fourth, 4, has left it a conſiderable time ago.

As ſoon as one of theſe ſatellites paſſes into the ſhade, it becomes inviſible, and that ſuddenly; ſo that at whatever place of the globe you may happen to be, the ſatellite which was before diſtinctly viſible, diſappears in an inſtant. This entrance of a ſatellite into the ſhadow of Jupiter is denominated *immerſion*, and it's departure from the ſhade *emerſion*; when the ſatellite, which had for ſome time been inviſible, ſuddenly re-appears.

The immerſions and emerſions are equally adapted to the determination of the longitude, as they take place at a decided inſtant; ſo that when ſuch a phenomenon is obſerved at ſeveral places of the globe, you muſt find, in the time indicated by the time-pieces of each, the difference which exactly correſponds to the difference of the diſtance of their meridians. Is is the ſame thing as if we obſerved the beginning or the end of an eclipſe of the moon; and the caſe is then involved in no difficulty. For ſome time paſt we have been able to calculate theſe eclipſes of the ſatellites of Jupiter, that is their immerſions and emerſions; and we have only to compare the time obſerved, with the time calculated for a given place, ſay Berlin, in order to conclude, at once, the diſtance of it's meridian from that of our capital.

This method is accordingly practiſed univerſally in travelling by land: but the means have not yet been diſcovered of profiting by it at ſea, where, however, it is of ſtill greater importance for a man to know with certainty where he is. Were the ſatellites of Jupiter as viſible to the naked eye as the moon is, this method would be attended with no difficulty, even at ſea, but the obſervation cannot be made without a teleſcope of at leaſt four or five feet in length, a circumſtance which preſents an inſurmountable obſtacle.

You well know that it requires ſome addreſs to manage, even at land, a teleſcope of any length, to direct it toward the object which you wiſh to contemplate, and to keep it ſo ſteady as not to loſe the object: you will eaſily comprehend, then, that a ſhip at ſea, being in a continual agitation, it muſt be almoſt impoſſible to catch Jupiter himſelf; and if you could find him, you would loſe him again in a moment. Now in order to make an accurate obſervation of the immerſion or emerſion of one of the ſatellites of Jupiter, it is abſolutely neceſſary that you ſhould have it in your power to look at him ſteadily for ſome time together, and this being impoſſible at ſea, we are, to all appearance, conſtrained to abandon this method of determining the longitude.

This inconvenience, however, may be remedied two ways; the one by the conſtruction of teleſcopes ſix inches long, or ſtill leſs, capable of diſcovering clearly the ſatellites of Jupiter; and there can be no doubt that theſe would be more manageable than ſuch as are four or five feet in length. Artiſts are actually employing themſelves with ſucceſs in bringing teleſcopes of this ſort to perfection; but it has not yet been proved whether or not it will require as much addreſs to point them to the object, as thoſe which are longer.

The other way would be to contrive a chair, to be uſed on ſhip-board, which ſhould remain fixed and motionleſs, ſo as not to be affected by the agitation of the veſſel. It does not ſeem impoſſible that a dextrous mode of balancing might effect this. In fact, it is not long ſince we read in the public prints, that an Engliſhman pretended that he had conſtructed ſuch a chair, and thereupon claimed the prize propoſed for the diſcovery of the longitude. His claim was well founded, if he indeed conſtructed the machine, as it would be poſſible, by means of it, to obſerve at ſea the immerſions and emerſions of the ſatellites of Jupiter, which are undoubtedly very much adapted to the making of this diſcovery: but for ſome time paſt no farther mention has been made of it. From the whole, you muſt have perceived to how many difficulties the diſcovery of the longitude is ſubjected.

26th September, 1761.

LETTER LII.

The Motion of the Moon, a Fifth Method.

THE heavens furniſh us with one reſource more for diſcovering the longitude without the aſſiſtance of teleſcopes, in which aſtronomers ſeem to place the greateſt confidence. It is the moon, not only when eclipſed, but at all times, provided ſhe be viſible; an unſpeakable advantage, conſidering that eclipſes are ſo rare, and that the immerſions and emerſions of the ſatellites of Jupiter are of ſuch difficult obſervation; there being a conſiderable time every year, during which the planet Jupiter is not viſible to us, whereas the moon is almoſt conſtantly in view.

You muſt undoubtedly have already remarked, that the moon riſes every day almoſt three quarters of an hour later than the preceding, not being attached to one fixed place relatively to the ſtars, which always preſerve the ſame ſituation with reſpect to each other, though they have the appearance of being carried round by the heavens, to accompliſh every day their revolution about the earth. I ſpeak here according to appearances; for it is the earth which revolves every day round it's axis, while the heavens and the fixed ſtars remain at reſt; while the ſun and planets are continually changing their place relatively to theſe. The moon has likewiſe a motion abundantly rapid from one day to another, with relation to the fixed ſtars,

If you were to ſee the moon to-day near a certain fixed ſtar, it will appear to-morrow, at the ſame hour, at a conſiderable diſtance from it, toward the eaſt, and the diſtance ſometimes exceeds even 15 degrees. The velocity of her motion is not always the ſame, yet we are able to determine it very exactly for every day; by which means we can calculate before-hand her true place in the heavens, for every hour of the day, and for any known meridian, ſay that of Berlin, or Paris.

Suppoſe, then, that after a long voyage I find myſelf at ſea, in a place altogether unknown, what uſe can I make of the moon, in order to diſcover the longitude of the place where I am? There is no difficulty with reſpect to the latitude, even at ſea, where there are means abundantly certain for aſcertaining the height of the pole, to which the latitude is always equal. My whole attention, then, will be directed to the moon; I will compare her with the fixed ſtars which are neareſt, and thence calculate her true place relatively to them. You know there are celeſtial globes, on which all the fixed ſtars are arranged, and that celeſtial charts are likewiſe conſtructed, ſimilar to geographical maps, on which are repreſented the fixed ſtars which appear in a certain quarter of the heavens. On taking, then, a celeſtial chart, on which the fixed ſtars, to which the moon is near, are marked, it will be an eaſy matter to determine the true place where the moon at that time is; and my watch, which I have taken care to regulate there, from an obſervation of the moment of noon, will indicate to me the time of my lunar obſervation. Then, from my knowledge of the moon's motion, I calculate for Berlin, at what hour ſhe muſt appear in the ſame place where I have ſeen her. If the time obſerved exactly correſpond with the time of Berlin, it will be a demonſtration, that the place where I am is preciſely under the meridian of Berlin, and that conſequently the longitude is the ſame. But if the time of my obſervation is not that of Berlin, the difference will give that which is between the meridians; and reckoning 15 degrees for every hour of time, I compute how much the longitude of the place I am at is greater or leſs than that of Berlin: the place where time is more advanced has always the greater longitude.

This is an abſtract of the manner of determining longitude by ſimple obſervations of the moon. I remark, that the happieſt moments for ſucceſsfully performing this operation, and for accurately determining the moon's place, are, when a fixed ſtar happens to be concealed behind her body; this is called *occultation*, and there are two inſtances favourable to obſervation, that when the moon in her motion completely covers the ſtar, and that when the ſtar re-appears. Aſtronomers are particularly attentive to catch theſe inſtants of occultation, in order to calculate from them the moon's true place.

I foreſee, however, an objection you will probably make, reſpecting the time-piece with which I ſuppoſe our navigator provided, after having maintained the impoſſibility of conſtructing one that ſhall be proof againſt every agitation of a ſhip at ſea. But this impoſſibility reſpects only ſuch time-pieces as are expected to preſerve a regular motion for a long time together, without the neceſſity of frequent adjuſtment: for as to the obſervations in queſtion, a common watch is quite ſufficient, provided it go regularly for ſome hours, after having been carefully adjuſted to the noon of the place where we are: ſuppoſing a doubt to ariſe, whether we could calculate from it the ſucceeding evening or night, at the time we obſerve the moon, the ſtars likewiſe will afford the means of a new and accurate adjuſtment. For as the ſituation of the ſun, with relation to the fixed ſtars, is perfectly known, for any time whatever, the ſimple obſervation of any one ſtar is ſufficient to determine the place where the ſun muſt then be; from which we are enabled to calculate the hour that a well-regulated time-piece ought to indicate. Thus, at the very inſtant of making an obſervation by the moon, we are enabled likewiſe to regulate our time-piece by the ſtars, and every time-piece is ſuppoſed to go regularly for ſo ſhort a ſpace.

29th September, 1761.

LETTER LIII.

Advantages of this laſt Method; its Degree of Preciſion.

THIS laſt method of finding the longitude, founded on lunar obſervations, ſeems to merit the preference, as the others are ſubjected to too many difficulties, or the opportunities of employing them occur too ſeldom, to be uſeful. And you muſt be abundantly ſenſible, that ſucceſs depends entirely on the degree of preciſion attained in forming the calculation, and that the errors which may be committed would lead to concluſions on which we could place no dependence. It is of importance, therefore, to explain what degree of preciſion we may reaſonably hope to be attainable in reducing this method to practice, founded on the conſiderable change which the moon undergoes, from one day to another, in her poſition. It may be affirmed, that if the moon's motion were more rapid, it would be more adapted to the diſcovery of the longitude, and would procure for us a higher degree of preciſion. But if, on the contrary, it were much ſlower, ſo that we could ſcarcely diſcern any change of her poſition from day to day, we could derive very little, if any, aſſiſtance from her, toward the diſcovery of the longitude.

Let us ſuppoſe, then, that the moon changes her place among the fixed ſtars, a ſpace of 12 degrees in 24 hours; ſhe will, in that caſe, change it one degree in two hours, and half a degree, or 30 minutes, in an hour: if we were to commit a miſtake in obſerving the moon's place, of 30 minutes, it would be the ſame thing as if we obſerved the moon an hour earlier or later, and we ſhould commit a miſtake of one hour in the concluſion, reſpecting the difference of the meridians. Now, one hour's difference in the meridians correſponds to 15 degrees in their longitude; conſequently, we ſhould be miſtaken 15 degrees in the longitude itſelf of the place we look for; which would undoubtedly be an error ſo enormous, that it were almoſt as well to know nothing about it; and a ſimple computation of the diſtance and the direction, however uncertain, could not poſſibly lead to a miſtake ſo very groſs. But a man muſt have gone to work in a very ſlovenly manner, to commit a miſtake of 30 minutes reſpecting the moon's place, and the inſtruments which he employed muſt have been very bad, a thing not to be ſuppoſed.

Nevertheleſs, however excellent the inſtruments may be, and whatever degree of attention may have been beſtowed, it is impoſſible to keep clear of all error, and he muſt have acquitted himſelf very well indeed, who has not committed the miſtake of one minute in determining the moon's place. Now, as it changes half a degree, or 30 minutes, in one hour, it will change one minute of diſtance in two minutes of time. When, therefore, the miſtake of the moon's place amounts to no more than one minute, the miſtake in the difference of meridians will amount to two minutes of time. And one hour, or 60 minutes, being equivalent to 15 degrees of longitude, there will reſult from it an error of half a degree in the longitude, and this point of preciſion might be ſufficient for every purpoſe, were it but attainable.

I have hitherto ſuppoſed our knowledge of the moon's motion to be ſo perfect, that, for a known meridian, we could determine the moon's true place, for every moment, without an error; but we are ſtill very far ſhort of that point of perfection. Within theſe twenty years, the error in this calculation was more than ſix minutes; and it is but lately that the ingenious Profeſſor *Mayer*, at Gottingen, purſuing the track I had pointed out to him, has ſucceeded ſo far as to reduce this error to leſs than a minute. It may very eaſily happen, then, that in the calculation likewiſe, the error of one minute may be committed; which, added to that of a minute committed in the obſervation of the moon's place, will double that which reſults from it, reſpecting the longitude of the place where we are; and, conſequently, it may poſſibly amount to a whole degree: it is proper farther to remark, that if the moon in 24 hours ſhould change her relative ſituation more than 12 degrees, the error in the longitude would be leſs conſiderable. The means may perhaps be diſcovered of diminiſhing ſtill farther the errors into which we are liable to fall, in the obſervation and in the calculation; and then we ſhould be able to aſcertain the longitude to a degree, or leſs. Nay, we ought not to deſpair of attaining a ſtill higher degree of preciſion. We have only to make ſeveral obſervations, which can be eaſily done by remaining ſeveral days together at the ſame place. It is not to be apprehended, in that caſe, that all the concluſions ſhould be equally defective; ſome will give the longitude ſought too great, others too ſmall, and by ſtriking a medium between all the concluſions, we may reſt aſſured that this longitude will not be one degree removed from the truth.

The Engliſh nation, generouſly diſpoſed to engage genius and ability in this important reſearch, has propoſed three prizes, for aſcertaining the longitude, one of £. 10,000, one of £. 15,000, and one of £. 20,000. The firſt of theſe is to be beſtowed on the perſon who ſhall determine the longitude to a degree, or about it; ſo as to give perfect aſſurance that the error ſhall not exceed one degree at moſt. The ſecond is to be given to him who ſhall diſcover a method ſtill more exact, ſo as that the error ſhall never exceed two thirds of a degree, or 40 minutes. The higheſt prize is deſtined to the man who ſhall aſcertain the longitude ſo exactly that the error ſhall never exceed half a degree, or 30 minutes: and a higher degree of preciſion is hardly to be expected. No one of theſe prizes has hitherto been allotted: I do not take into the account the gratification beſtowed on the artiſt who pretended to it from his conſtruction of perfect time-pieces. Mr. *Mayer* is at this moment claiming the higheſt, and I think he is entitled to it.

3d October, 1761.

LETTER LIV.

On the Mariner's Compaſs, and the Properties of the Magnetic Needle.

YOU are by this time ſufficiently informed reſpecting the diſcovery of the longitude: I have had the pleaſure of explaining the various methods which have been employed for the determination of it.

The firſt, and moſt natural, is carefully to obſerve the quantity of ſpace which we have gone over, and the direction in which we moved; but the currents and tempeſts to which ſea-voyages are expoſed, render this method impracticable.

The ſecond requires the conſtruction of a time-piece ſo perfect as to go always uniformly, notwithſtanding the agitation of a ſhip at ſea; which no artiſt has hitherto been able to accompliſh.

The third is founded on obſervation of the eclipſes of the moon, which would completely anſwer every purpoſe, were not opportunities of employing it too rare, and leaſt in our power, when the neceſſity may be moſt urgent.

The fourth refers to the eclipſes of the ſatellites of Jupiter, which would anſwer the purpoſe extremely well, had we the means of employing, at ſea, teleſcopes of a certain deſcription, without which they are inviſible.

Finally, obſervations of the moon herſelf furniſh a fifth method, which appears the moſt practicable, provided we were able to obſerve the moon's place in the heavens ſo exactly, that the error in calculation (and error is unavoidable) ſhould never exceed one minute, in order to be aſſured that we are not miſtaken above one degree in the determination of the longitude.

To one or the other of theſe five methods, perſons engaged in this reſearch have chiefly directed their ſpeculations; but there is ſtill a ſixth, which ſeems likewiſe adapted to the ſolution of the problem, were it more carefully cultivated; and will perhaps one day furniſh us with the moſt certain method of diſcovering the longitude; though as yet we are far, very far, ſhort of it.

It is not derived from the heavens, but is attached to the earth ſimply, being founded on the nature of the magnet, and of the compaſs. The explication of it opens to me a new field of important phyſical obſervation, for your amuſement and inſtruction, on the ſubject of magnetiſm, and I flatter myſelf you will attend with delight and improvement to the elucidations which I am going to ſuggeſt.

My reflections ſhall be directed only to the main ſubject of our preſent reſearch, I mean the diſcovery of the longitude. I remark, in general, that the magnet is a ſtone which has the quality of attracting iron, and of diſpoſing itſelf in a certain direction; and that it communicates the ſame quality to iron and ſteel, by rubbing, or ſimply touching them with a magnet; propoſing afterwards to enter into a more minute diſcuſſion of this quality, and to explain the nature of it.

I begin, then, with the deſcription of a magnetic needle, which, mounted in a certain manner, for the uſe of mariners, is denominated the compaſs.

For this purpoſe, we provide a needle of good ſteel, nearly reſembling *fig.* 2. of *plate II.* one extremity of which B terminates in a point, the better to diſtinguiſh it from the other A; it is furniſhed at the middle C with a ſmall cap, hollowed below, for the purpoſe of placing the needle on a pivot or point D, as may be ſeen in the ſecond figure.

The two ends are adjuſted in ſuch a manner, that the needle, being in perfect equilibrium, can revolve freely, or remain at reſt, on the pivot, in whatever ſituation it may be placed. Before the magnet is ap-

plied,

plied, it would be proper to temper the needle, in order to render it as hard as poſſible; then by rubbing or touching it with a good loadſtone, it will inſtantly acquire the magnetic virtue. The two extremities will no longer balance each other, but the one D will deſcend, as if it had become heavier; and in order to reſtore the equilibrium, ſomething muſt be taken away from the extremity B, or a ſmall weight added to the end A. But the artiſts, foreſeeing this change produced by magnetiſm, make the end B originally lighter than the end A, that the magnetized needle may of itſelf aſſume the horizontal poſition.

It then acquires another property ſtill much more remarkable; it is no longer indifferent to all ſituations, as formerly; but affects one in preference to every other, and diſpoſes itſelf in ſuch a manner that the extremity B is directed to the north nearly, and the extremity A toward the ſouth; and the direction of the magnetic needle correſponds almoſt with the meridian line.

You recollect that, in order to trace a meridian line, which may point out the north and the ſouth, it is neceſſary to have recourſe to aſtronomical obſervations, as the motion of the ſun and ſtars determines that direction; and when we are not provided with the neceſſary inſtruments, and eſpecially when the ſky is overclouded, it is impoſſible to derive any aſſiſtance from the heavens toward tracing the meridian line; this property of the magnetic needle is, therefore, ſo much the more admirable, that it points out, at all times, and in every place, the northern direction, on which depend the others, toward the eaſt, ſouth and weſt. For this reaſon the uſe of the magnetic needle, or compaſs, is become univerſal.

It is in navigation that the advantages reſulting from the uſe of the compaſs are moſt conſpicuous; it being always neceſſary to direct the courſe of a veſſel toward a certain quarter of the world, in order to reach a place propoſed, conformably to geographic or marine charts, which indicate the direction in which we ought to proceed. Before this diſcovery, accordingly, it was impoſſible to undertake long voyages; the mariner durſt not loſe ſight of the coaſt, for fear of miſtaking his courſe, unleſs the ſky was unclouded, and the ſtars pointed out the way.

A veſſel on the wide ocean, without the knowledge of the proper courſe, would be preciſely in the ſtate of a man who, with a bandage over his eyes, was obliged to find his way to the great church of Magdeburg; imagining he was going one way, he might be going another. The compaſs, then, is the principal guide in navigation, and it was not till after this important diſcovery that men ventured acroſs the ocean, and attempted the diſcovery of a new world. What could a pilot do, without his compaſs, during or after a ſtorm, when he could derive no aſſiſtance from the heavens? Take whatever courſe he might, he muſt be ignorant in what direction he was proceeding, north, ſouth, or to any other quarter. He would preſently deviate to ſuch a degree, as infallibly to loſe himſelf. But the compaſs immediately puts him right; from which you will be enabled to judge of the importance of the diſcovery of the magnetic needle, or mariner's compaſs.

6th October, 1761.

LETTER LV.

Declination of the Compaſs, and Manner of obſerving it.

THOUGH the magnetic needle affects the ſituation of being directed from ſouth to north, there are accidental cauſes capable of deranging this direction, which muſt be carefully avoided. Such are the proximity of a loadſtone, or of iron, or ſteel. You have only to preſent a knife to a magnetic needle, and it will immediately quit it's natural direction, and move toward the knife; and, by drawing the knife round the needle, you will make it aſſume every poſſible direction. In order to be aſſured, then, that the needle is in it's natural direction, you muſt keep at a diſtance from it all iron or ſteel, as well as magnets; which is ſo much the more eaſy, that theſe ſubſtances influence it's direction only when very near it: once removed, their effect becomes inſenſible, unleſs in the caſe of a very powerful magnet, which might poſſibly act on the needle at the diſtance of ſeveral feet.

But iron alone produces not this effect, as the compaſs may be uſed to advantage even in iron-mines. You are perfectly ſenſible, that under ground, in mines, we are in the ſame condition as at ſea, when the face of heaven is overclouded, and that it is neceſſary to drive mines in a certain direction. Plans are accordingly conſtructed repreſenting all the tracks hollowed out in the bowels of the earth, and this operation is regulated merely by the compaſs: this is the object of the ſcience denominated ſubterraneous geometry.

To return to our compaſs, or magnetic needle, I have remarked that it's direction is only almoſt northerly; it is therefore incorrect to ſay that the magnet has the property of always pointing north. Having employed myſelf in the fabrication of many magnetic needles, I ever found that their direction at Berlin deviated about fifteen degrees from the true meridian line; now, an aberration of 15° is very conſiderable.

Figure 3, *plate II.* repreſents, firſt, the true meridian line drawn from north to ſouth; that which is drawn at right angles with it indicates the eaſt, to the right hand; and the weſt, to the left. Now the magnetic needle AB does not fall on the meridian, but deviates from it an angle of 15°, BO *north*. This angle is denominated the *declination*, and ſometimes the *variation* of the compaſs or magnetic needle: and as the extremity B, neareſt the north, deviates toward the weſt, we ſay, the declination is 15° weſterly.

Having thus determined the declination of the magnetic needle, we can make it anſwer the ſame purpoſe as if it pointed directly north. The needle is uſually incloſed in a circle, and you have only to mark on it the due north at the exact diſtance from the northern extremity of the needle, ſo as to make a declination of 15° weſtward, and the line north-ſouth will indicate the true meridian line, and enable us to aſcertain the four cardinal points, north, eaſt, ſouth and weſt.

The better to diſguiſe the ſecret, the magnetic needle is concealed in a circle of paſteboard, as repreſented in the figure, only the needle is rendered inviſible, the paſteboard covering it, and forming but one body with it, the centre of which is placed on a pivot, in order to admit of a free revolution; it aſſumes of courſe a ſituation ſuch that the point marked *north* is always directed to that point of the horizon; whereas the needle, which is not ſeen, in effect deviates from it 15° to the weſt. This conſtruction ſerves only to diſguiſe the declination, which the vulgar conſider as a defect, though it be rather an object worthy of admiration, as we ſhall afterwards ſee; and the paſteboard only increaſing the weight of the needle, prevents it's turning ſo freely as if it were unencumbered.

To remedy this, and more commodiouſly to employ the compaſs, the needle is depoſited in a circular box, the circumference of which, divided into 360°, exhibits the name of the principal points of the horizon. In the centre is the pivot or point which ſupports the needle, and this laſt immediately aſſumes a certain direction; the box is then turned till the northern extremity of the needle B exactly correſponds with the fifteenth degree on the circumference, reckoning from the north-weſtward; and then the names marked will agree with the real quarters of the world.

At ſea, however, they employ needles caſed in circles of paſteboard, the circumference of which is divided into 360 degrees, to prevent the neceſſity of turning round the box; then the paſteboard circle, which is called the compaſs, indicating the real quarters of the world, we have only to refer to it the courſe which the ſhip is ſteering, in order to aſcertain the direction, whether north or ſouth, eaſt or weſt, or any other intermediate point. By the compaſs likewiſe we diſtinguiſh the winds, or the quarters from which they blow, and from the points marked on it their names are impoſed. It is neceſſary, at any rate, to be perfectly aſſured of the declination or variation of the compaſs; we have found it to be exactly 15° weſtward here at Berlin; but it may be different at other places, as I ſhall afterwards demonſtrate.

10th October, 1761.

LETTER LVI.

Difference in the Declination of the Compaſs at the ſame Place.

WHEN I ſay that the declination of the compaſs is 15 degrees weſtward, this is to be underſtood as applying only to Berlin, and the preſent time; for it has been remarked, that not only nada, Florida, the Antilles, and toward the coaſts of Brazil. Beyond theſe countries, toward the eaſt, that is, toward Europe and Africa, it again becomes weſterly, as I have already remarked.

In order to attain a perfect knowledge of the preſent ſtate of magnetic declination, it would be neceſſary to aſcertain for all places, both at land and ſea, the preſent ſtate of magnetic declination, and whether it's tendency is weſtward or eaſtward. This knowledge would be undoubtedly extremely uſeful, but we dare ſcarcely hope for it. It would require men of ability, in every part of the globe, employed, at the ſame time, in obſerving, each on his own ſtation, the magnetic declination, and who ſhould communicate their obſervations with the utmoſt exactneſs. But the ſpace of ſome years would elapſe before the communications of the more remote could be received; thus the knowledge aimed at is unattainable till after the expiration of years. Now, though no very conſiderable change takes place in the direction of the magnetic needle in two or three years, this change, however ſmall, would however prevent the attainment of complete information reſpecting the preſent ſtate of the various declinations of the magnetic needle, from obſervations made, at the ſame time, in the different regions of the globe.

The ſame thing holds with reſpect to times paſt; to every year correſponds a certain ſtate of magnetic declination proper to itſelf, and which diſtinguiſhes it from every other period of time, paſt and future. It were, however, ſincerely to be wiſhed, that we had an exactly detailed ſtate of the declination for one year only; the moſt important elucidations of the ſubject would certainly be derived from it.

The late Mr. *Halley*, a celebrated Engliſh aſtronomer, has attempted to do this for the year 1700, founding his concluſions on a great number of obſervations made at different places, both by land and ſea; but beſide that ſome very conſiderable diſtricts, where theſe obſervations were not made, are not taken into his account, moſt of thoſe which he has employed, were made ſeveral years prior to 1700; ſo that at this era the declination might have undergone very conſiderable alterations. It follows, that this ſtatement, which we find repreſented on a general chart of the earth, muſt be conſidered as extremely defective; and, moreover, what would it now avail us to know the ſtate of magnetic declination for the year 1700, having ſince then undergone a conſiderable change?

Other Engliſh geographers have produced, poſterior to that period, a ſimilar chart, intended to repreſent all the declinations, ſuch as they were in the year 1744. But as it has the ſame defect with that of Mr. *Halley*; and as they likewiſe were unable to procure obſervations from ſeveral countries on the globe, they did not ſcruple to fill up the vacant places, by conſulting *Halley*'s chart, which certainly could not apply to 1744. You will conclude, from what I have ſaid, that our knowledge of this important branch of phyſics is ſtill extremely imperfect.

13th October, 1761.

LETTER LVII.

Chart of Declinations; Method of employing it for the Diſcovery of the Longitude.

IT may be proper, likewiſe, to explain in what manner *Halley* proceeded to repreſent the magnetic declinations, in the chart which he conſtructed for the year 1700, that if you ſhould happen to ſee it, you may comprehend it's ſtructure.

Firſt, he marked, at every place, the declination of the magnetic needle, ſuch as it had been there obſerved. He diſtinguiſhed, among all theſe places, thoſe where there was no declination, and found that they all fall in a certain line, which he calls the line of no declination, as every where under that line there was then none. This line was neither a meridian nor a parallel, but run in a very oblique direction over North America, and left it near the coaſts of Carolina; thence it bent it's courſe acroſs the Atlantic Ocean between Africa and America. Beſide this line he diſcovered likewiſe another, in which the declination diſappeared; it deſcended through the middle of China, and paſſed from thence through the Philippine Iſles and New Holland. It is eaſy to judge, from the track of theſe two lines, that they have a communication near both poles of the globe.

Having fixed theſe two lines of no declination, Mr. *Halley* remarked that, every where between the firſt and laſt, proceeding from weſt to eaſt, that is through all Europe, Africa, and almoſt the whole of Aſia, the declination was weſterly; and that on the other ſide, between thoſe lines, that is, over the whole Pacific Ocean, it was eaſterly. After this, he obſerved all the places in which the declination was 5 degrees weſt, and found he could ſtill conveniently draw a line through all theſe places, which he calls the line of five degrees weſt. He found likewiſe two lines of this deſcription, the one of which accompanied, as it were, the firſt of no declination, and the other the laſt. He went on in the ſame manner with the places where the declination was 10°; afterwards 15°, 20°, &c. and he ſaw that theſe lines of great declination were confined to the polar regions; whereas thoſe of ſmall declination encompaſſed the whole globe, and paſſed through the equator.

In fact, the declination ſcarcely ever exceeds 15° on the equator, whether weſt or eaſt; but on approaching the poles, it is poſſible to arrive at places, where the declination exceeds 58° and 60°. There are undoubtedly ſome, where it is ſtill greater, exceeding even 90°, and where the northern extremity of the needle will conſequently turn about and point ſouthward.

Finally, having drawn ſimilar lines through the places where the declination was eaſtward 10°, 15°, 20°, and ſo on, Mr. *Halley* filled up the whole chart, which repreſented the entire ſurface of the earth, under each of which lines the declination is univerſally the ſame, provided the obſervations are not erroneous.

roneous. Mr. *Halley* has, accordingly, ſcrupulouſly abſtained from continuing ſuch lines beyond the places where obſervations had actually been made: for this reaſon the greater part of his chart is a blank.

Had we ſuch a chart, accurate and complete, we ſhould ſee, at a glance, what declination muſt have predominated at each place, at the time for which the chart was conſtructed; and though the place, in queſtion, ſhould not be found preciſely under one of the lines traced on the chart, by comparing it with the two lines between which it might be ſituated, we could eaſily calculate the intermediate declination which correſponded to it. If I found my preſent place to be between the lines of 10° and 15° of weſtern declination, I ſhould be certain that the declination there was more than 10°, and leſs than 15°; and according as I might be nearer the one or the other, I could eaſily find the juſt medium, which would indicate the true declination.

From this you will readily comprehend, that if we had ſuch a chart, thus exact, it would aſſiſt us in diſcovering longitude, at leaſt for the time to which it correſponded. In order to explain this method, let us ſuppoſe that we are poſſeſſed of a chart conſtructed for the preſent year, we would ſee on it, firſt, the two lines drawn through the places where there is no declination; then the two where it is 5°, 10°, 15°, 20°, both eaſt and weſt: let us farther ſuppoſe that, for the greater exactneſs, theſe lines were drawn from degree to degree, and that I found myſelf at a certain place on ſea, or in an unknown country, I would in the firſt place draw a meridian line, in order to aſcertain how much my compaſs deviated from it, and I ſhould find, for example, that the declination is preciſely 10° eaſt; I ſhould then take my chart, and look for the two lines under which the declination is 10° eaſt, fully aſſured that I am under the one or the other of theſe two lines, which muſt at once greatly relieve my uncertainty. Finally, I would obſerve the height of the pole, which being the latitude of my place, nothing more would remain but to mark, on the two lines mentioned, the points where the latitude is the ſame with that which I have juſt obſerved; and then all my uncertainty is reduced to two points very diſtant from each other; now, the circumſtances of my voyage would eaſily determine which of thoſe two places is that where I actually am.

You will admit that if we had charts ſuch as I have deſcribed, this method would be the moſt commodious and accurate of all, for aſcertaining the longitude: but this is preciſely the thing we want; and as we are ſtill very far from having it in our power to conſtruct one for the time paſt, which would be of no uſe for the preſent time, for want of a ſufficient number of obſervations, we are ſtill leſs inſtructed reſpecting all the changes of declination which every place undergoes in the lapſe of time. The obſervations hitherto made aſſure us, that certain places are ſubject to very conſiderable variations, and that others ſcarcely undergo any, in the ſame interval of time; which ſtrips us of all hope of ever being able to profit by this method, however excellent it may be in itſelf.

17th October, 1761.

LETTER LVIII.

Why does the Magnetic Needle affect, in every Place of the Earth, a certain Direction, differing in different Places; and for what Reaſon does it change, with Time, at the ſame Place?

YOU will undoubtedly have the curioſity to be informed, why magnetic needles affect, at every place on the globe, a certain direction; why this direction is not the ſame at different places; and why, at the ſame place, it changes with the courſe of time? I ſhall anſwer theſe important enquiries to the beſt of my ability, though I fear not ſo much to your ſatisfaction as I could wiſh.

I remark, firſt, that magnetic needles have this property in common with all magnets, and that it is only their form, contrived to balance and revolve freely on a pivot, which renders it more conſpicuous. The loadſtone, ſuſpended by a thread, turns toward a certain quarter, and when put in a ſmall veſſel to make it ſwim on water, the veſſel which ſupports the loadſtone will always affect a certain direction. Every loadſtone fitted with two oppoſite points, the one of which is directed to the north, and the other to the ſouth, will be ſubject to the ſame variations as the magnetic needles.

Theſe points are very remarkable in all loadſtones, as by them iron is attracted with the greateſt force.

They are denominated the *poles* of a loadſtone, a term borrowed from that of the poles of the earth or of the heavens; becauſe the one has a tendency toward the north and the other toward the ſouth pole of the earth; but this is to be underſtood as only almoſt, not exactly, the caſe; for when the name was impoſed, the declination had not yet been obſerved. That pole of the loadſtone which is directed northward is called it's north pole, and that which points ſouthward it's ſouth pole.

I have already remarked, that a magnetic needle, as well as the loadſtone itſelf, aſſumes this ſituation, which appears natural to it, only when removed from the vicinity of another loadſtone, or of iron. When a magnetic needle is placed near a loadſtone, it's ſituation is regulated by the poles of that loadſtone; ſo that the north pole of the loadſtone attracts the ſouthern extremity of the needle, and reciprocally the ſouth pole of the loadſtone the northern extremity of the needle. For this reaſon, in referring one loadſtone to another, we call thoſe the friendly poles which bear different names, and thoſe the hoſtile which have the ſame name. This property is ſingularly remarkable on bringing two loadſtones near each other: for then we find that not only do the poles of different names mutually attract, but that thoſe of the ſame name ſhun and repel each

other.

[illegible] s is ſtill more conſpicuous when two r[illegible]ıetic needles are brought within the ſphere of ı[illegible]ience.

[illegible] ord[illegible] :o be ſenſible of this, it is of much importance [illegible] conſider the ſituation which a magnetic needle aſſumes in the vicinity of a loadſtone.

The bar AB, *(plate II. fig. 4.)* repreſents a loadſtone, wh[illegible] north pole is B, and the ſouth pole A: you ſee v[illegible]ıſi[illegible]s [illegible] the magnetic needle, under the fig[illegible]ɔf [illegible]ırrow[illegible] ſe extremity marked *b* is the n[illegible], the ſouth. In all theſe poſitions, the [illegible]mity *b* of the needle is directed toward the pole A of the loadſtone; and the extremity *a* to the pole B. The point *c* indicates the pivot on which the needle revolves; and you have only to conſider the figure with ſome attention, in order to determine what ſituation the needle will aſſume, in whatever poſition round the loadſtone the pivot *c* is fixed.

If there were, therefore, any where, a very large loadſtone AB, the magnetic needles placed round it would aſſume, at every place, a certain ſituation, as we ſee actually to be the caſe round the globe. Now if the globe itſelf were that loadſtone, we ſhould comprehend why the magnetic needles every where aſſumed a certain direction. Naturaliſts, accordingly, in order to explain this phenomenon, maintain that the whole globe has the property of a magnet, or that we ought to conſider it as a prodigious loadſtone. Some of them allege, that there is at the centre of the earth a very large loadſtone, which has exerciſed it's influence on all the magnetic needles, and even on all the loadſtones, which are to be found on the ſurface of the earth; and that it is this influence which directs them in every place, conformably to the directions which we obſerve them to aſſume.

But there is no occaſion to have recourſe to a loadſtone concealed in the bowels of the earth. It's ſurface is ſo repleniſhed with mines of iron and loadſtone, that their united force may well ſupply the want of this huge magnet. In fact, all loadſtones are extracted from mines, an infallible proof that theſe ſubſtances are found in great abundance in the bowels of the earth, and that the union of all their powers furniſhes the general force, which produces all the magnetical phenomena. We are likewiſe enabled thereby to explain, wherefore the magnetic declination changes, with time, at the ſame place; for it is well known, that mines of every kind of metal are ſubject to perpetual change, and particularly thoſe of iron, to which the loadſtone is to be referred. Sometimes iron is generated, and is ſometimes deſtroyed at one and the ſame place; there are accordingly, at this day, mines of iron where there were none formerly; and where it was formerly found in great abundance, there are now hardly any traces of it. This is a ſufficient proof, that the total maſs of loadſtones contained in the earth is undergoing very conſiderable changes, and thereby, undoubtedly, the poles, by which the magnetic declination is regulated, likewiſe change with the lapſe of time.

Here then we muſt look for the reaſon, why the magnetic declination is ſubject to changes ſo conſiderable at the ſame places of the globe. But this very reaſon, founded on the inconſtancy of what is paſſing in it's bowels, affords no hope of our ever being able to aſcertain the magnetic declination beforehand, unleſs we could find the means of ſubjecting the changes of the earth to ſome fixed law. A long ſeries of obſervations, carried on through ſeveral ages ſucceſſively, might poſſibly throw ſome light on the ſubject.

20th October, 1761.

LETTER LIX.

Elucidations reſpecting the Cauſe and Variation of the Declination of Magnetic Needles.

THOSE who allege that the earth contains in it's womb a prodigious loadſtone, like a ſtone with a kernel in fruit, are under the neceſſity of admitting, in order to explain the magnetic declination, that this ſtone is ſucceſſively ſhifting it's ſituation. It muſt in that caſe be detached from the earth in all it's parts; and as it's motion would undoubtedly follow a certain law, we might flatter ourſelves with the hope of one day diſcovering it. But whether there be ſuch a magnetic ſtone within the earth, or whether the loadſtones ſcattered up and down through it's entrails unite their force to produce the magnetical phenomena, we may always conſider the earth itſelf as a loadſtone, in ſubſerviency to which every particular loadſtone, and all magnetic needles, aſſume their direction.

Certain naturaliſts have encloſed a very powerful magnet in a globe, and having placed a magnetic needle on it's ſurface, obſerved phenomena ſimilar to thoſe which take place on the globe of the earth, by placing the magnet within the globe, in ſeveral different poſitions. Now, conſidering the earth as a loadſtone, it will have it's magnetic poles, which muſt be carefully diſtinguiſhed from the natural poles, round which it revolves. Theſe poles have nothing in common between them but the name; but it is from the poſition of the magnetic poles, relatively to the natural, that the apparent irregularities in the magnetic declination proceed, and particularly of the lines traced on the globe, of which I have endeavoured to give you ſome account.

In order more clearly to elucidate this ſubject, I remark, that if the magnetic poles exactly coincided with the natural, there would be no declination all over the earth: magnetic needles would univerſally point to the north preciſely, and their poſition would be exactly that of the meridian line. This were no doubt an unſpeakable advantage in navigation, as we ſhould then know with preciſion the courſe of the veſſel and the direction of the wind; whereas, at preſent, we muſt always look for the declination of the compaſs before we are able to determine the true quarters of the world. But then the compaſs could furniſh no aſſiſtance toward aſcertaining the longitude, an object which the declination may ſooner or later render attainable.

Hence it may be concluded, that if the magnetic poles of the earth differed very greatly from the natural, and that if they were directly oppoſite to each other, which would be the caſe, if the magnetic axis of the earth, that is the ſtraight line drawn from the one magnetic pole to the other, paſſed through the centre of the earth, then magnetic needles would univerſally point toward theſe magnetic poles, and it would be eaſy to aſſign the magnetic direction proper to every place; we ſhould only have to draw for every place a circle which ſhould at the ſame time paſs through the two magnetic poles, and the angle which this circle would make with the meridian of the ſame place muſt give the magnetic declination.

In this caſe, the two lines, under which there is no declination, would be the meridians drawn through the magnetic poles. But as we have ſeen that, in reality, theſe two lines without declination are not meridians, but take a very unaccountable direction, it is evident that no ſuch caſe actually takes place. *Halley* clearly ſaw this difficulty, and therefore thought himſelf obliged to ſuppoſe a double loadſtone in the bowels of the earth, the one fixed, the other moveable; of conſequence, he was obliged to admit four poles of the earth, two of them toward the north, and two toward the ſouth, at unequal diſtances.

But this hypotheſis ſeems to me rather a bold conjecture: it by no means follows, that becauſe theſe lines of no declination are not meridians, there muſt be four magnetic poles on the earth: but rather, that there are only two, which are not directly oppoſite to each other; or, which comes to the ſame thing, that the magnetic axis does not paſs through the centre of the earth.

It remains, therefore, that we conſider the caſes in which theſe two magnetic poles are not directly oppoſite, and in which the magnetic axis does not paſs through the centre of the earth; for if we embrace the hypotheſis of the magnetic nut within the earth, why ſhould one of it's poles be preciſely oppoſite to the other? This nut may very probably be not exactly in the very centre of the earth, but at a conſiderable diſtance from it. Now, if the magnetic poles are not diametrically oppoſite to each other, the lines of no declination may actually aſſume a direction ſimilar to that which, from obſervation, we find they do; it is even poſſible to aſſign to the two magnetic poles ſuch places on the earth, that not only theſe lines ſhould coincide with obſervation, but likewiſe, for every degree of declination, whether weſtern or eaſtern, we may find lines preciſely ſimilar to thoſe which, at firſt, ſeemed ſo unaccountable.

In order, then, to know the ſtate of magnetic declination, all that is requiſite, is to fix the two magnetic poles; and then it becomes a problem in geometry, to determine the direction of all the lines which I mentioned in my preceding letter, drawn for every place where the declination is the ſame; by ſuch means too we ſhould be enabled to rectify theſe lines, and to fill up the countries where no obſervations have been made: and were it poſſible to aſſign, for every future period, the places of the two magnetic poles on the globe, it would undoubtedly prove the moſt ſatisfying ſolution of the problem of the longitude.

There is no occaſion, therefore, for a double loadſtone within the earth, or for four magnetic poles, in order to explain the declination of magnetic needles, as *Halley* ſuppoſed, but for a ſimple magnet, or two magnetic poles, provided it's juſt place is aſſigned to each. It appears to me that, from this reflection, we are much more advanced in our knowledge of magnetiſm.

24th October, 1761.

LETTER LX.

Inclination of Magnetic Needles.

YOU will pleaſe to recollect, that on rubbing a needle againſt the loadſtone it acquires not only the property of pointing toward a certain point of the horizon, but that it's northern extremity ſinks, as if it had become heavier, which obliges us to diminiſh it's weight ſomewhat, or to increaſe that of the other extremity, in order to reſtore the equilibrium. I have, without putting this in practice, made ſeveral experiments to aſcertain how far the magnetic force brought down the northern extremity of the magnetized needle, and I have found that it ſunk ſo as to make an angle of 72 degrees with the horizon, and that in this ſituation the needle remained at reſt. It is proper to remark, that theſe experiments were made at Berlin, about ſix years ago; for I ſhall ſhew you afterwards, that this direction to below the horizon, is as variable as the magnetic declination.

Hence we ſee that the magnetic power produces a double effect on needles; the one directs the needle toward a certain quarter of the horizon, the deviation of which from the meridian line is what we call the magnetic declination; the other impreſſes on it an inclination toward the horizon, ſinking the one or the other extremity under it, up to a certain angle.

Let *d e (plate II. fig. 5.)* be the horizontal line, drawn according to the magnetic declination, and the needle will aſſume, at Berlin, the ſituation *b a*, which makes with the horizon *d e* the angle *d c b* or *e c a*, which is 72°, and conſequently, with the vertical *f g*, an angle *b c g* or *a c f* of 18 degrees. This ſecond effect of the magnetic force, by which the magnetic needle affects a certain inclination toward the horizon, is as remarkable as the firſt; and as the firſt is denominated the magnetic *declination*, the ſecond is known by the name of magnetic *inclination*, which deſerves, as well as the declination, to be every where obſerved with all poſſible care, as we find in it a ſimilar variety.

The

The inclination at Berlin has been found 72°, at Bâle only 70°, the northern extremity of the needle being ſunk, and the oppoſite, of conſequence, raiſed to that angle. This takes place in countries which are nearer to the northern magnetic pole of the earth; and in proportion as we approach it, the greater becomes the inclination of the magnetic needle, or the more it approaches the vertical line; ſo that if we could reach the pole itſelf, the needle would there actually aſſume a vertical ſituation, it's northern extremity pointing perpendicularly downward, and its ſouthern end upward. The farther, on the contrary, you remove from the northern magnetic pole of the earth, and approach the ſouthern, the more the inclination diminiſhes; it will at length diſappear, and the needle will aſſume a horizontal poſition, when equally diſtant from both poles: but in proceeding toward the ſouth pole of the earth, the ſouthern extremity of the needle will ſink more and more under the horizon, the northern extremity riſing in proportion, till at the pole itſelf, the needle again becomes vertical, with the ſouthern extremity perpendicularly downward, and the northern upward.

It were devoutly to be wiſhed that experiments had been as carefully, and as generally made, in the view of aſcertaining the magnetic inclination, as of determining the declination; but this important article of experimental philoſophy has hitherto been too much neglected, though certainly neither leſs curious, nor leſs intereſting, than that of the declination.

tion. It is not however a matter of ſurprize; experiments of this ſort are ſubject to too many difficulties; and almoſt all the methods hitherto attempted of obſerving the magnetic inclination have failed. One artiſt alone, Mr. *Diterich* of Bâle, has ſucceeded, having actually conſtructed a machine proper for the purpoſe, under the direction of the celebrated Mr. *Daniel Bernouilli.* He ſent me two of theſe machines, by means of which I have obſerved, at Berlin, this inclination of 72 degrees; and however curious, in other reſpects, the Engliſh and French may be, in proſecuting ſuch enquiries, they have put no great value on Mr. *Diterich*'s machine, though the only one adapted to the deſign. This inſtance demonſtrates how the progreſs of ſcience may be obſtructed by prejudice; hence Berlin and Bâle are the only two places on the globe, where the magnetic inclination is known.

Needles prepared for the conſtruction of compaſſes are by no means proper to indicate the quantity of magnetic inclination, though they may convey a rough idea of it's effect, becauſe the northern extremity in theſe latitudes becomes heavier. In order to render ſerviceable needles intended to diſcover the declination, we are under the neceſſity of deſtroying the effect of the inclination, by diminiſhing the weight of the northern extremity, or increaſing that of the ſouthern. To reſtore the needle to a horizontal poſition, the laſt of theſe methods is uſually employed, and a ſmall morſel of wax is affixed to the ſouthern extremity of the needle. You are abundantly

be aſcertained, provided the inclination
ition are known.

nagnet exhibits phenomena altogether
ver ou have only to place one on a table co-
filings of ſteel, and you will ſee the filings
emſelves round the loadſtone AB, nearly
ted in *fig.* 6, *plate II.* in which every par-
filings may be conſidered as a ſmall mag-
le, indicating, very point round the
, the magnetic di ion. This experiment
ie to inquire into the cauſe of all theſe pheno-
mena.

The arrangement aſſumed by the ſteel filings leaves no room to doubt that it is a ſubtile and inviſible matter which runs through the particles of the ſteel, and diſpoſes them in the direction which we here obſerve. It is equally clear that this ſubtile matter pervades the loadſtone itſelf, entering at one of the poles, and going out at the other: ſo as to form, by it's continual motion round the loadſtone, a vortex which re-conducts the ſubtile matter from one pole to the other, and this motion is, without doubt, extremely rapid.

The nature of the loadſtone conſiſts, then, in a continual vortex, which diſtinguiſhes it from all other bodies; and the earth itſelf, in quality of loadſtone, muſt be ſurrounded with a ſimilar vortex, acting every where on magnetic needles, and making continual efforts to diſpoſe them according to its own direction, which is the ſame I formerly denominated the magnetic direction: this ſubtile matter is continually iſſuing, then, at one of the magnetic poles of the earth, and after having performed a circuit round to the other pole, it there enters, and pervades the globe through and through to the oppoſite pole, where it again eſcapes.

We are not yet enabled to determine by which of the two magnetic poles of the earth it enters or iſſues: the phenomena depending on this have ſuch a perfect reſemblance, that they are indiſtinguiſhable. It is undoubtedly, likewiſe, this general vortex of the globe which ſupplies the ſubtile matter of every particular loadſtone to magnetic iron or ſteel, and which keeps up the particular vortices that ſurround them.

In order to a thorough inveſtigation of the nature of this ſubtile matter, and it's motion, it muſt be remarked, that it's action is confined to loadſtone, iron and ſteel; all other bodies are abſolutely indifferent to it; the relation which it bears to thoſe muſt, therefore, be by no means the ſame which it bears to others. We are warranted to maintain, from manifold experiments, that this ſubtile matter freely pervades all other bodies, and even in all directions: for, when a loadſtone acts on a needle, the action is perfectly the ſame whether another body interpoſes or not, provided the interpoſing body is not iron, and it's action is the ſame on the filings of iron. This ſubtile matter, therefore, muſt pervade all bodies, iron excepted, as freely as it does air, and even pure ether; for theſe experiments ſucceed equally well in a receiver exhauſted by the air-pump. This matter is conſequently different from ether, and even much more ſubtile. And, on account of the general vortex of the earth, it may be affirmed that the globe is completely ſurrounded by it, and freely pervaded, as all other bodies are, excepting the loadſtone and iron: for this reaſon, iron and ſteel may be denominated magnetic bodies, to diſtinguiſh them from others.

But if this magnetic matter paſſes freely through all non-magnetic bodies, what relation can it have to thoſe which are ſuch? We have juſt obſerved that the magnetic vortex enters at one of the poles of every loadſtone, and goes out at the other; whence it may be concluded, that it freely pervades loadſtones likewiſe; which would not diſtinguiſh them from other bodies. But as the magnetic matter paſſes through the loadſtone only from pole to pole, this is a circumſtance very different from what takes place in others. Here, then, we have the diſtinctive character. Non-magnetic bodies are freely pervaded by the magnetic matter, in all directions: loadſtones are pervaded by it, in one direction only; one of the poles being adapted to it's admiſſion, the other to it's eſcape. But iron and ſteel, when rendered magnetic, fulfil this laſt condition; when they are not, it may be affirmed, that they do not grant a free tranſmiſſion to the magnetic matter, in any direction.

This may appear ſtrange, as iron has open pores, which tranſmit the ether, though it is not ſo ſubtile as the magnetic matter. But we muſt carefully diſtinguiſh a ſimple paſſage, from one in which the magnetic matter may pervade the body, with all it's rapidity, without encountering any obſtacle.

31ſt October, 1761.

LETTER LXII.

Nature of the Magnetic Matter, and of it's rapid Current. Magnetic Canals.

I AM very far from pretending to explain perfectly the phenomena of magnetiſm; it preſents difficulties which I did not find in thoſe of electricity. The cauſe of it undoubtedly is, that electricity conſiſts in a too great, or too ſmall, degree of compreſſion, of a ſubtile fluid which occupies the pores of bodies, without ſuppoſing that ſubtile fluid, which is the ether, to be in actual motion; but magnetiſm cannot be explained, unleſs we ſuppoſe a vortex in rapid agitation, which penetrates magnetic bodies.

The matter which conſtitutes theſe vortices is likewiſe much more ſubtile than ether, and freely pervades the pores of loadſtones, which are impervious even to ether. Now, this magnetic matter is diffuſed through, and mixed with, the ether, as the ether is with groſs air, or juſt as ether occupies and fills up the pores of air, it may be affirmed that the magnetic matter occupies and fills the pores of ether.

I conceive, then, that loadſtone and iron have pores ſo ſmall that the ether in a body connot force it's way into them, and that the magnetic matter alone can penetrate them; and which, on being admitted, ſeparates itſelf from the ether, by what may be called a kind of filtration. In the pores of the loadſtone alone, therefore, is the magnetic matter to be found in perfect purity: every where elſe it is blended with ether, as this laſt is with the air.

You can eaſily imagine a ſeries of fluids, one always more ſubtile than another, and which are perfectly blended together. Nature furniſhes inſtances of this. Water, we know, contains in it's pores particles of air, which are frequently ſeen diſcharging themſelves in the form of ſmall bubbles: air again, it is equally certain, contains in it's pores a fluid incomparably more ſubtile, namely ether, and which, on many occaſions, is ſeparated from it, as in electricity. And now we ſee a ſtill farther progreſſion, and that ether contains a matter much more ſubtile than itſelf, the magnetic matter, which may, perhaps, contain, in it's turn, others ſtill more ſubtile, at leaſt this is not impoſſible.

Having ſettled this magnetic matter, let us ſee how it's phenomena are produced. I conſider a loadſtone, then, and ſay, firſt, that beſides a great many pores filled with ether, like all other bodies, it contains ſome ſtill much more narrow, into which the magnetic matter alone can find admiſſion. Secondly, theſe pores are diſpoſed in ſuch a manner as to have a communication with each other, and conſtitute tubes or canals, through which the magnetic matter paſſes from the one extremity to the other. Finally, this matter can be tranſmitted through theſe tubes only in one direction, without the poſſibility of returning in an oppoſite direction. This moſt eſſential circumſtance requires a more particular elucidation.

Firſt, then, I remark, that the veins and lymphatic veſſels in the bodies of animals, are tubes of a ſimilar conſtruction, containing valves, repreſented in *fig.* 7, *plate II.* by the ſtrokes *m n*, whoſe office it is to grant, by raiſing themſelves, a free paſſage to the blood when it flows from A to B, and to prevent it's reflux from B to A. For if the blood attempted to flow from B to A, it would preſs down the moveable extremity of the valve *m* on the ſide of the vein *o*, and totally obſtruct the paſſage. Valves are thus employed in aqueducts, to prevent the reflux of the water. I do not conſider myſelf, then, as ſuppoſing any thing contrary to nature, when I ſay, that the canals, in loadſtones, which admit the magnetic matter only, are of the ſame conſtruction.

Figure 8, *plate II.* repreſents this magnetic canal, according to my idea of it. I conceive it furniſhed inwardly with briſtles directed from A toward B, which preſent no oppoſition to the magnetic matter in it's paſſage from A to B, for in this caſe they open of themſelves at *n*, to let the matter paſs at *o*; but they would immediately obſtruct the channel, were it to attempt a retrograde courſe from B to A. The nature of magnetic canals conſiſts, then, in granting admiſſion to the magnetic matter only at A, to flow toward B, without the poſſibility of returning in the oppoſite direction from B toward A.

This

This conſtruction enables us to explain how the magnetic matter enters into theſe tubes, and flies through them with the greateſt rapidity, even when the whole ether is in a ſtate of perfect reſt, which is the moſt ſurpriſing: for how can a motion ſo rapid be produced? This will appear perfectly clear to you, if you will pleaſe to recollect that ether is a matter extremely elaſtic; accordingly the magnetic matter, which is ſcattered about, will be preſſed by it on every ſide. Let us ſuppoſe the magnetic canal A B ſtill quite empty, and that a particle of magnetic matter *m* preſents itſelf at the entrance A, and this particle preſſed on every ſide at the opening of the canal, into which the ether cannot force admiſſion, it will there be preſſed forward with prodigious force, and enter into the canal with equal rapidity; another particle of magnetic matter will immediately preſent itſelf, and be driven forward with the ſame force, and in like manner all the following particles. There will thence reſult a continual flux of magnetic matter, which, meeting with no obſtruction in this canal, will eſcape from it at B, with the ſame rapidity that it enters at A.

My idea, then, is, that every loadſtone contains a great multitude of theſe canals, which I denominate magnetic; and it very naturally follows, that the magnetic matter diſperſed in the ether muſt enter into them at one extremity, and eſcape at the other, with great impetuoſity; that is, we ſhall have a perpetual current of magnetic matter through the canals

of

Fig. 2.

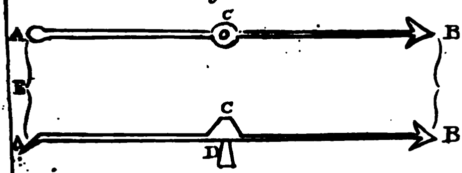


Fig 3

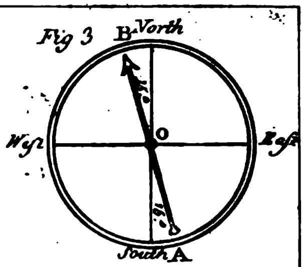


Fig. 1

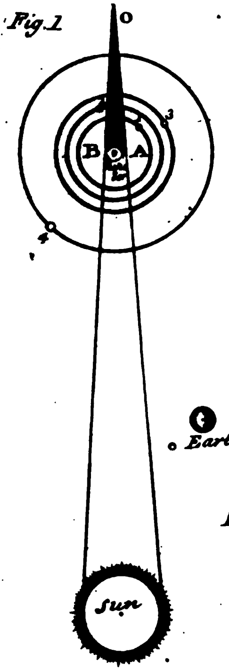


Fig. 4.

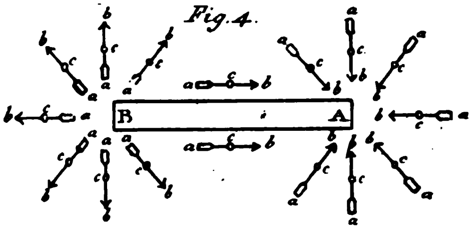


Fig. 5.

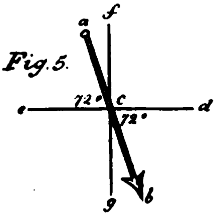


Fig. 6

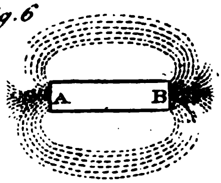


Fig. 7

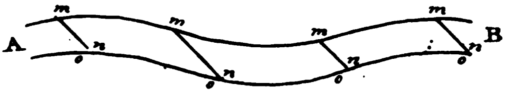
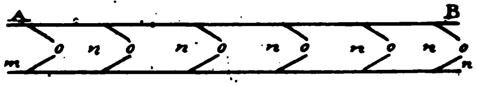


Fig. 8



of the loadſtone: and thus I hope I have ſurmounted the greateſt difficulties which can occur in the theory of magnetiſm.

3d *November*, 1761.

LETTER LXIII.

Magnetic Vortex. Action of Magnets upon each other.

YOU have now ſeen in what the diſtinctive character of the loadſtone conſiſts; and that each contains ſeveral canals, of which I have attempted a deſcription.

Figure 1. *plate III.* repreſents a loadſtone A B, with three magnetic canals *a b*, through which the magnetic matter will flow with the utmoſt rapidity, entering at the extremities marked *a*, and eſcaping at thoſe marked *b*: it will eſcape indeed with the ſame rapidity, but immediately meeting with the ether blended with the groſſer air, great obſtructions will oppoſe the continuation of it's motion in the ſame direction; and not only will the motion be retarded, but it's direction diverted toward the ſides *c c*. The ſame thing will take place at the entrance, toward the extremities *a a a*; on account of the rapidity with which the particles of magnetic matter force their way into them, the circulation will quickly overtake thoſe which are ſtill toward the ſides *e e*, and theſe, in their turn, will be replaced by thoſe which, eſcaped from the extremities *b b b*, have been already diverted toward *c c*; ſo that the ſame magnetic matter which iſſued from the extremities *b b b* quickly returns toward thoſe marked *a a a*, performing the circuit *b c d e a*, and this circulation, round the loadſtone, is what we call the *magnetic vortex*.

It muſt not be imagined, however, that it is always the ſame magnetic matter, which forms theſe vortices; a conſiderable part of it will eſcape, no doubt, as well toward B as toward the ſides, in performing the circuit; but as a compenſation, freſh magnetic matter will enter by the extremities *a a a*, ſo that the matter which conſtitutes the vortex is ſuccedaneous and very variable: a magnetic vortex, ſurrounding the loadſtone, will, however, always be kept up, and produce the phenomena formerly obſerved in filings of ſteel, ſcattered round the loadſtone.

You will pleaſe farther to attend to this circumſtance, that the motion of the magnetic matter in the vortex, is incomparably ſlower out of the loadſtone, than in the magnetic tubes, where it is ſeparated from the ether, after having been forced into them by all the elaſtic power of this laſt fluid; and that, on eſcaping, it mixes again with the ether, and thereby loſes great part of it's motion, ſo that it's velocity in travelling to the extremities *a a a* is incomparably leſs than in the magnetic canals *a b*, though ſtill very great with reſpect to us. You will eaſily comprehend, then, that the extremities of the magnetic canals, by which the matter enters into the loadſtone and eſcapes from it, are what we call it's poles; and that the magnetic poles of a loadſtone are by no means mathematical points, the whole ſpace, in which the extremities of the magnetic canals terminate, being one magnetic pole, as in the loadſtone repreſented *figure* 6, *plate II.* where the whole ſurfaces A and B are the two poles.

Now, as theſe poles are diſtinguiſhed by the terms north and ſouth, yet we cannot affirm with certainty whether it is by the north or ſouth pole that the magnetic matter enters into loadſtones. You will ſee in the ſequel, that all the phenomena produced by the admiſſion and eſcape, have ſuch a perfect reſemblance, that it appears impoſſible to determine the queſtion by experiments. It is, therefore, a matter of indifference, whether we ſuppoſe that the magnetic matter enters or eſcapes by the north pole or by the ſouth.

Be it as it may, I ſhall mark with the letter A, the pole by which the magnetic matter enters, and with B, that by which it eſcapes, without pretending thereby to indicate which is north or ſouth. I proceed to the conſideration of theſe vortices, in order to form a judgment, how two loadſtones act upon each other.

Let us ſuppoſe (*plate III. fig.* 2.) that the two loadſtones A B and *a b* are preſented to each other by the poles of the ſame name A, *a*, and their vortices will be in a ſtate of total oppoſition. The magnetic matter which is at C will enter at A and *a*, and theſe two vortices attempting mutually to deſtroy each other, the matter which proceeds by E to enter at A will meet at D that of the other loadſtone, returning by *e* to enter at *a*: from this muſt reſult a colliſion of the two vortices, in which the one will repel the other; and this effect will extend to the loadſtones themſelves, which, thus ſituated, undergo mutual repulſion. The ſame thing would take place, if the two loadſtones preſented to each other the other poles B and *b*: for this reaſon the poles of the ſame name are denominated *hoſtile*, becauſe they actually repel each other.

But if the loadſtones preſent to each other the poles of a different name, an oppoſite effect will enſue, and you will perceive that they have a mutual attraction.

In *figure* 3, *plate III.* where the two loadſtones preſent to each other the poles B and *a*, the magnetic matter which iſſues from the pole B, finding immediately free admiſſion into the other loadſtone by it's pole *a*, will not be diverted toward the ſides, in order to return and re-enter at A, but will paſs directly by C into the other loadſtone, and eſcape from it at *b*, and will perform the circuit by the ſides *d d* to re-enter, not by the pole *a*, but by the pole A, of the other loadſtone, completing the circuit by *e f*. Thus the vortices of theſe two loadſtones will unite, as if there were but one; and this vortex being compreſſed on all ſides by the ether, will impel the two loadſtones toward each other, ſo that they will exhibit a mutual attraction.

This is the reaſon why the poles of different names are denominated *friendly*, and thoſe of the ſame name *hoſtile*, the principal phenomenon in magnetiſm, in as much as the poles of different names attract, and thoſe of the ſame name repel each other.

7th November, 1761.

LETTER LXIV.

Nature of Iron and Steel. Manner of communicating to them the Magnetic Force.

HAVING ſettled the nature of the loadſtone in theſe canals which the magnetic matter can pervade in only one direction, becauſe the valves they contain prevent it's return in the contrary direction, you can no longer doubt that they are the continuation of thoſe pores, (*fig.* 8, *plate II.*) whoſe fibres point in the ſame direction, ſo that ſeveral of theſe particles, being joined in continuation, conſtitute one magnetic canal. It is not ſufficient, therefore, that the matter of the loadſtone ſhould contain many ſimilar particles; they muſt likewiſe be diſpoſed in ſuch a manner as to form canals continued from one extremity to the other, in order to grant an uninterrupted tranſmiſſion to the magnetic matter.

Iron and ſteel, then, apparently contain ſuch particles in great abundance; theſe are not, however, originally diſpoſed in the manner I have been deſcribing, but are ſcattered over the whole maſs, and this diſpoſition is all they want to become real magnets. In that caſe, they ſtill retain all their other qualities, and are not diſtinguiſhable from other maſſes of iron and ſteel, except that now they have, beſides, the properties of the loadſtone: a knife and a needle anſwer the ſame purpoſes, whether they have or want the magnetic virtue. The change which takes place in the interior, from the arrangement of the particles in the order which magnetiſm requires, is not externally perceptible; and the iron or ſteel which has acquired the magnetic force, is denominated an artificial magnet, to diſtinguiſh it from the natural, which reſembles a ſtone, though the magnetic properties are the ſame in both. You will have a curioſity, no doubt, to be informed in what manner iron and ſteel may be brought to receive the magnetic force, and ſo become artificial magnets. Nothing can be more ſimple; and the vicinity of a loadſtone is capable of rendering iron ſomewhat magnetic: it is the magnetic vortex which produces this effect, even though the iron and loadſtone ſhould not come into contact.

However hard iron may appear, the particles which contain the magnetic pores formerly repreſented, are very pliant in ſubſtance, and the ſmalleſt force is ſufficient to change their ſituation. The magnetic matter of the vortex, entering into the iron, will then eaſily diſpoſe the firſt magnetic pores which it meets, following it's own direction; thoſe at leaſt whoſe ſituation is not very different; and having run through them, it will act in the ſame manner on the adjacent pores, till it has forced a paſſage quite through the iron, and thereby formed ſome magnetic canals. The figure of the iron contributes greatly to facilitate this change; a lengthened figure, and placed in the ſame direction with the vortex, is moſt adapted to it, as the magnetic matter, in paſſing through the whole length, there diſpoſes a great many particles in their juſt ſituation, in order to form longer magnetic canals; and it is certain, that the more there is the means of forming canals, the longer they will be without interruption, the more rapid will be the motion of the magnetic matter, and the greater the magnetic force.

It has likewiſe been remarked, that when the iron, placed in a magnetic vortex, is violently ſhaken or ſtruck, it acquires a higher degree of magnetiſm from this, becauſe the minute particles are by ſuch concuſſion agitated and diſengaged, ſo as to yield more eaſily to the action of the magnetic matter which penetrates them.

Placing accordingly a ſmall bar of iron *a b* (*plate III. fig.* 4.) in the vortex of the loadſtone A B, ſo that it's direction may nearly agree with that of the current *d e f* of the magnetic matter, it will with eaſe paſs through the bar, and form in it magnetic canals, eſpecially if, at the ſame time, the bar is ſhaken or ſtruck, to facilitate the tranſmiſſion. It is likewiſe obſervable, that the magnetic matter, which enters at the pole A of the loadſtone, and eſcapes at the pole B, will enter the bar at the extremity *a* and eſcape at the extremity *b*, ſo that the extremity *a* will become the pole of the ſame name A, and *b* the ſame with B. Then taking this bar *a b* out of the magnetic vortex, it will be an artificial

though very feeble, which will ſupply it tex, and preſerve it's magnetic power, as long as it's magnetic canals ſhall not be interrupted. This will take place ſo much the more eaſily that the pores of iron are pliant; thus the ſame circumſtance which aſſiſts the production of magnetiſm, contributes likewiſe to it's deſtruction. A natural magnet is not ſo eaſily enfeebled, becauſe the pores are much cloſer, and more conſiderable efforts are requiſite to derange them. I ſhall go more largely into the detail afterward.

I here propoſe to explain the manner of moſt naturally rendering iron magnetic; though the force which it thence acquires is very ſmall, it will aſſiſt us in comprehending this remarkable and almoſt univerſal phenomenon. It has been obſerved, that the tongs and other implements of iron which are uſually placed in a vertical poſition, as well as bars of iron fixed perpendicularly on ſteeples, acquire in time a very ſenſible magnetic force. It has likewiſe been perceived, that a bar of iron, hammered in a vertical poſition, or heated red hot, on being plunged into cold water in the ſame poſition, becomes ſomewhat magnetic, without the application of any loadſtone.

In order to account for this phenomenon, you have only to recollect that the earth itſelf is a loadſtone, and conſequently encompaſſed with a magnetic vortex, of which the declination and inclination of the magnetic needle every where ſhew the true direction. If then a bar of iron remain long in that poſition, there is no reaſon to be ſurprized, ſhould it become magnetic. We have likewiſe ſeen, that the inclination of the magnetic needle is, at Berlin, 72 degrees;

and as it is nearly the ſame all over Europe, this inclination differs only 18 degrees from the vertical poſition; the vertical poſition, accordingly, differs but little from the direction of the magnetic vortex: a bar of iron, long kept in that poſition, will be at laſt penetrated with the magnetic vortex, and muſt conſequently acquire a magnetic force.

In other countries, where the inclination is imperceptible, which is the caſe near the equator, it is not the vertical, but rather the horizontal poſition which renders bars of iron magnetic, for their poſition muſt correſpond to the magnetic inclination, if you would have them acquire a magnetic force. I ſpeak here only of iron; ſteel is too hard for the purpoſe, and means more efficacious muſt be employed to communicate the magnetic virtue to it.

10th November, 1761.

LETTER LXV.

Action of Loadſtones on Iron. Phenomena obſervable on placing Pieces of Iron near a Loadſtone.

THOUGH the whole earth may be conſidered as a vaſt loadſtone, and as encompaſſed with a magnetic vortex, which every where directs the magnetic needle, it's magnetic power is, however, very feeble, and much leſs than that of a very ſmall loadſtone: this appears very ſtrange, conſidering the enormous magnitude of the earth.

It ariſes, undoubtedly, from our very remote diſtance from the real magnetic poles of the earth, which, from every appearance, are buried at a great depth below the ſurface: now, however powerful a loadſtone may be, it's force is conſiderable only when it is very near; and as it removes that force gradually diminiſhes, and at length diſappears. For this reaſon, the magnetic force acquired in time by maſſes of iron ſuitably placed in the earth's vortex is very ſmall, and indeed hardly perceptible, unleſs it is very ſoft, and of a figure adapted to the production of a vortex, as has been already remarked.

This effect is much more conſiderable near a loadſtone of moderate ſize: ſmall maſſes of iron ſpeedily acquire from it a very perceptible magnetic force; they are likewiſe attracted toward the loadſtone; whereas this effect is imperceptible in the earth's vortex, and conſiſts only in directing magnetic needles, without either attracting them or increaſing their weight.

A maſs of iron plunged into the vortex of a loadſtone, likewiſe preſents very curious phenomena, which well deſerve a particular explanation. Not only is this maſs at firſt attracted toward the loadſtone, but it too attracts other pieces of iron. Let AB, *(plate III. fig. 5.)* be a natural magnet, in the vicinity of which, at the pole B, is placed the maſs of iron CD, and it will be found that this laſt is capable of ſupporting a bar of iron EF. Apply again to this, at F, an iron ruler GH, in any poſition whatever, ſay horizontal, ſupporting it at H, and it will be found that the ruler is not only attracted by the bar at F, but likewiſe capable of ſupporting, at H, needles as I K, and that theſe needles again act on filings of iron L, and attract them.

The magnetic force may thus be propagated to very conſiderable diſtances, and even made to change it's direction, by the different poſition of theſe pieces of iron, though it gradually diminiſhes. You are perfectly ſenſible, that the more powerful the loadſtone AB is of itſelf, and the nearer to it the firſt maſs CD, the more conſiderable likewiſe is the effect. The late Mr. de *Maupertuis* had a large loadſtone ſo powerful, that at the diſtance even of ſeveral feet, the maſs of iron CD continued to exert a very conſiderable force.

In order to explain theſe phenomena, you have only to conſider, that the magnetic matter which eſcapes rapidly, at the pole of the loadſtone B, enters into the maſs of iron, and diſpoſes the pores of it to form magnetic canals, which it afterward freely pervades. In like manner, on entering into the bar, it will there too form magnetic canals, and ſo on. And as the magnetic matter, on iſſuing from one body, enters into another, theſe two bodies muſt undergo a mutual attraction, for the ſame reaſon, as I have before proved, that two loadſtones, which preſent their friendly poles to each other, muſt be attracted: and as often as we obſerve an attraction between two pieces of iron, we may with certainty conclude, that the magnetic matter which iſſues from the one is entering into the other, from the continual motion with which it penetrates theſe bodies. It is thus that, in the preceding diſpoſition of the bars of iron, the magnetic matter in it's motion pervades all of them, and this is the only reaſon of their mutual attraction.

The ſame phenomena ſtill preſent themſelves, on turning the other pole A of the loadſtone, by which the magnetic matter enters, toward the maſs of iron. The motion in this caſe becomes retrograde, and preſerves the ſame courſe; for the magnetic matter contained in the maſs of iron will then eſcape from it, to paſs rapidly into the loadſtone, and, in making it's eſcape, will employ the ſame efforts to arrange the pores in the maſs ſuitably to the current, as if it were rapidly entering into the iron. To this end, therefore, the iron muſt be ſufficiently ſoft, and theſe pores pliant, to obey the efforts of the magnetic matter. A difficulty will, no doubt, here occur to you; it will be aſked, How do you account for the change of direction of the magnetic matter, on entering into another bar of iron; and why is that direction regulated according to the length of the bars, as it's courſe is repreſented in the figure? This is a very important article in the theory of magnetiſm, and it proves how much the figure of the pieces of iron contributes to the production of the magnetic phenomena.

To elucidate this, it muſt be recollected, that this ſubtile matter moves with the utmoſt eaſe in the magnetic pores, where it is ſeparated from the ether;

and that it encounters very conſiderable obſtacles, when it eſcapes from them, with all it's velocity, to re-enter into the ether and the air.

Let us ſuppoſe that the magnetic matter, after having pervaded the bar CD, (*fig.* 6. *plate III.*) enters into the iron ruler EF, placed perpendicularly. It would certainly, on it's firſt admiſſion, preſerve the ſame direction, in order to eſcape at *m*, unleſs it found an eaſier road in which to continue it's motion: but meeting at *m* the greateſt obſtruction, it at firſt changes it's direction, though in a very ſmall degree, toward F, where finding pores adapted to the continuation of it's motion, it will deviate more and more from it's firſt direction, and travel through the ruler EF in all it's length; and, as if the magnetic matter were loth to leave the iron, it endeavours to continue it's motion there as long as poſſible, availing itſelf of the length of the ruler; but if the ruler were very ſhort, it would undoubtedly eſcape at *m*. But the length of the ruler preſenting it a ſpace to run through, it follows the direction EF, till it is under the neceſſity of eſcaping at F, where all the magnetic canals, formed according to the ſame direction, no longer permit the ſubtile magnetic matter to change it's direction, and return along the ruler; theſe canals being not only filled with ſucceeding matter, but, from their very nature, incapable of receiving motion in an oppoſite direction.

14th November, 1761.

LETTER LXVI.

Arming of Loadſtones.

YOU have juſt ſeen how iron may receive the magnetic current of a loadſtone, convey it to conſiderable diſtances, and even change it's direction. To unite a loadſtone, therefore, to pieces of iron, is much the ſame with increaſing it's ſize, as the iron acquires the ſame nature with reſpect to the magnetic matter; and it being farther poſſible by ſuch means to change the direction of the magnetic current, as the poles are the places where this matter enters the loadſtone and eſcapes, we are enabled to conduct the poles at pleaſure.

On this principle is founded the arming, or mounting, of loadſtones; a ſubject well worthy of your attention, as loadſtones are thereby carried to a higher degree of ſtrength.

Loadſtones, on being taken from the mine, are uſually reduced to the figure of a parallelopiped, or rectangular parallelogram, with thickneſs as AA, BB, (*fig.* 7. *plate III.*) of which the ſurface AA is the pole by which the magnetic matter enters, and BB that by which it eſcapes. It is filled, then, the whole length AB with canals *a b*, which the magnetic matter, impelled by the elaſtic power of the ether, freely pervades in the utmoſt rapidity, without any mixture of that fluid. Let us now ſee in what manner ſuch a loadſtone is uſually armed.

To each ſurface AA and BB, (*plate III. fig.* 8.) the two poles of the loadſtone, are applied plates of iron *a a* and *b b*, terminating below in the knobs 𝔄 and 𝔅, called the feet; this is what we denominate the armour of the loadſtone, and when this is done, the loadſtone is ſaid to be armed. In this ſtate, the magnetic matter which would have eſcaped at the ſurface BB, paſſes into the iron plate *b b*, where the difficulty of flying off into the air, in it's own direction, obliges it to take a different one, and to flow along the plate *b b* into the foot 𝔅, and there it is under the neceſſity of eſcaping, as it no longer finds iron to aſſiſt the continuation of it's motion. The ſame thing takes place on the other ſide; the ſubtile matter will be there conducted through the foot 𝔄, from which it will paſs into the plate *a a*, changing it's direction to enter into the loadſtone, and to fly through it's magnetic canals. For the ſubtile matter, contained in the plate, enters firſt into the loadſtone; it is followed by that which is the foot 𝔄, replaced in it's turn by the external magnetic matter, which being there impelled by the elaſticity of the ether, penetrates the foot 𝔄 and the plate *a a* with a rapidity whoſe vehemence is capable of arranging the poles, and of forming magnetic canals.

Hence it is evident that the motion muſt be the ſame on both ſides, with this difference, that the magnetic matter will enter by the foot 𝔄, and eſcape by the foot 𝔅, ſo that in theſe two feet we now find the poles of the armed loadſtone; and as the poles formerly diffuſed over the ſurfaces AA and BB are red for the conſtruction of ſuch armour, becauſe it's pores are pliant, and eaſily arrange themſelves in conformity to the current of the magnetic matter: iron of this deſcription, accordingly, appears much adapted to the production of a ſudden change in the direction of the current: the magnetic matter, too, ſeems to affect a progreſs in that direction as long as poſſible, and quits it not, till the continuance of it's motion through that medium is no longer practicable: it prefers making a circuit to a premature departure: a thing that does not take place in the loadſtone itſelf, in which the magnetic canals are already formed, nor in ſteel, whoſe pores do not ſo eaſily yield to the efforts of a magnetic current. But when theſe canals are once formed in ſteel, they are not ſo eaſily deranged, and much longer retain their magnetic force; whereas ſoft iron, whatever force it may have exerted during it's junction with a loadſtone, loſes it almoſt entirely on being disjoined.

Experience muſt be conſulted as to the other circumſtances of arming loadſtones. Reſpecting the plates, it has been found, that a thickneſs either too great or too ſmall is injurious; but for the moſt part, the beſt adapted plates are very thin, which would appear ſtrange, did we not know that the magnetic matter is much more ſubtile than ether, and that, conſequently, the thinneſt plate is ſufficient to receive a very great quantity of it.

17th November, 1761.

LETTER LXVII.

Action and Force of armed Loadſtones.

AT the feet of it's armour, then, a loadſtone exerts it's greateſt force, becauſe there it's poles are collected; and each foot is capable of ſupporting a weight of iron, greater or leſs in proportion to the excellency of the loadſtone.

Thus a loadſtone A A, B B, *(plate III. fig. 9.)* armed with plates of iron *a a* and *b b*, terminating in the feet 𝔄 and 𝔅, will ſupport by the foot 𝔄 not only the iron ruler CD, but this laſt will ſupport another of ſmaller ſize EF, this again another ſtill ſmaller GH, which will in it's turn ſupport a needle IK, which, finally, will attract filings of iron L; becauſe the magnetic matter runs through all theſe pieces to enter at the pole 𝔄; or if it were the other pole, by which the magnetic matter iſſues from the loadſtone, it would in like manner run through the pieces CD, EF, GH, IK. Now, as often as the matter is tranſmitted from one piece to another, an attraction between the two pieces is obſervable, or rather, they are impelled toward each other by the ſurrounding ether, becauſe the current of the magnetic matter between them diminiſhes the preſſure of that fluid.

When one of the poles of the loadſtone is thus loaded, it's vortex undergoes a very remarkable change of direction; for as, without this weight, the magnetic matter which iſſues from the pole 𝔅, directing around it's courſe, would flow toward the other pole 𝔄; and as now the entrance into this pole is ſufficiently ſupplied by the pieces which it ſupports, the matter iſſuing from the pole 𝔅 muſt take quite a different road, which will at length conduct it to the laſt piece IK. A portion of it will, undoubtedly, be likewiſe conveyed toward the laſt but one GH, and toward thoſe which precede it, as thoſe which follow, being ſmaller, do not ſupply in ſufficient abundance thoſe which go before, but the vortex will always extend to the laſt piece. By theſe means, if the pieces are well proportioned to each other, in length and thickneſs, the loadſtone is capable of ſupporting much more, than if it were loaded with a ſingle piece, in which the figure likewiſe enters principally into conſideration. But in order to make it ſuſtain the greateſt poſſible weight, we muſt contrive to unite the force of both poles.

For this purpoſe, there is applied to the two poles 𝔄 and 𝔅, (*plate IV. fig.* 1.) a piece of ſoft iron CD, touching the baſes of the feet in all points, and whoſe figure is ſuch, that the magnetic matter which iſſues from 𝔅 ſhall find in it the moſt commodious paſſage to re-enter at the other extremity 𝔄. Such a piece of iron is called the ſupporter of the loadſtone; and as the magnetic matter enters into it, on iſſuing from the loadſtone at 𝔅, and enters into the other pole 𝔄 on iſſuing from the ſupporter, the iron will be attracted to both poles at once, and conſequently with great force. In [illegible] to know how much power the loadſtone exerts, there is affixed to the ſupporter at the middle F, a weight P, which is increaſed till the loadſtone is no longer capable of ſuſtaining it, and then that weight is ſaid to counterbalance the magnetic power of the loadſtone: this is what you are to underſtand when told, that ſuch a loadſtone carries ten pounds weight, ſuch another thirty, and ſo on. Mahomet's coffin, they pretend, is ſupported by the force of a loadſtone; a thing by no means impoſſible, as artificial magnets have already been conſtructed which carry more than 100 pounds weight.

A loadſtone armed with it's ſupporter loſes nothing of the magnetic matter, which performs it's complete vortex within the loadſtone and the iron, ſo that none of it eſcapes into the air. Since, then, magnetiſm exerts it's power only in ſo far as the matter eſcapes from one body to enter into another; a loadſtone whoſe vortex is ſhut up, ſhould no where exert the magnetic power; nevertheleſs when it is touched on the plate at *a* with the point of a needle, a very powerful attraction is perceptible, becauſe the magnetic matter being obliged ſuddenly to change it's direction, in order to enter into the canals of the loadſtone, finds a more commodious paſſage by running through the needle, which will conſequently be attracted to the plate *a a*. But, by that very thing, the vortex will be deranged inwardly; it will not flow ſo copiouſly into the feet; and if you were to apply ſeveral needles to the plate, or iron rulers ſtill more powerful, the current toward the feet would be entirely diverted, and the force which attracts the ſupporter would altogether diſappear, ſo that it would drop off without effort. Hence it is evident, that the feet loſe their magnetic power in proportion as the loadſtone exerciſes it's force in other places, and thus we are enabled to account for a variety of very ſurprizing phenomena, which, without the theory, would be abſolutely inexplicable.

This is the proper place for introducing the experiment which demonſtrates, that after having applied it's ſupporter to an armed loadſtone, you may go on, from day to day, increaſing the weight which it is able to ſuſtain, till it, at length, ſhall exceed the double of what it carried at firſt. It is neceſſary to ſhew, therefore, how the magnetic force may, in time, be increaſed in the feet of the armour. The caſe above deſcribed, of the derangement of the vortex, aſſures us, that at the moment when the ſupporter is applied, the current of the magnetic matter is ſtill abundantly irregular, that a conſiderable part of it is ſtill eſcaping by the plate *b b*, and that it will require time to force magnetic canals in the iron; it is likewiſe probable that, when the current ſhall have become more free, new canals may be formed in the loadſtone itſelf, conſidering that it contains, beſide thoſe fixed canals, moveable poles, as iron does. But on violently ſeparating the ſupporter from the loadſtone, the current being diſturbed, and theſe new canals in a great meaſure deſtroyed, the force is ſuddenly rendered as ſmall as at the beginning; and ſome time muſt intervene before theſe canals,

nals, with the vortex, can recover their preceding ſtate. I once conſtructed an artificial magnet, which at firſt could ſupport only ten pound weight, and, after ſome time, I was ſurprized to find that it could ſupport more than thirty. It is remarked, chiefly in artificial magnets, that time alone ſtrengthens them conſiderably, but that this increaſe of force laſts only till the ſupporter is ſeparated from it.

21ſt November, 1761.

LETTER LXVIII.

The Manner of communicating to Steel the Magnetic Force, and of magnetizing Needles for the Compaſs: the Simple Touch, *it's Defects; Means of remedying theſe.*

HAVING explained the nature of magnets in general, an article as curious as intereſting ſtill remains, namely, the manner of communicating to iron, but eſpecially to ſteel, the magnetic power, and even the higheſt degree poſſible, of that power.

You have ſeen that, by placing iron in the vortex of a loadſtone, it acquires a magnetic force, but which almoſt totally diſappears, as ſoon as it is removed out of the vortex; and that the vortex of the earth alone is capable, in time, of impreſſing a ſlight magnetic power upon iron; now, ſteel being harder than iron, and almoſt entirely inſenſible to this action of the magnetic vortex, more powerful operations muſt

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employed to magnetize it; but then it retains the magnetic force much longer.

For this purpoſe we muſt have recourſe to touching, and even to friction. I begin, therefore, with explaining the method formerly employed, for magnetizing the needles of compaſſes; the whole operation conſiſted in rubbing them at the pole with a good loadſtone, whether naked or armed.

The needle *a b c (plate IV. fig. 2.)* was laid on a table; the pole B of the loadſtone was drawn over it, from *b* toward *a*, and, being arrived at the extremity *a*, the loadſtone was raiſed aloft, and brought back through the air to *b*; this operation was repeated ſeveral times together, particular care being taken that the other pole of the loadſtone ſhould not come near the needle, as this would have diſturbed the whole proceſs. Having ſeveral times drawn the pole B of the loadſtone over the needle from *b* to *a*, the needle had become magnetic, and the extremity *b* of the ſame name with that of the loadſtone with which it had been rubbed. In order to render the extremity *b* the north pole, it would have been neceſſary to rub with the pole of this name in the loadſtone, proceeding from *b* to *a*; but in rubbing with the ſouth pole, the progreſs muſt be from *a* to *b*.

This method of rubbing or touching, is denominated *the ſimple touch*, becauſe the operation is performed by touching with one pole only; but it is extremely defective, and communicates but very little power to the needle, let the loadſtone be ever ſo excellent; accordingly it does not ſucceed, when the ſteel is carried to the higheſt degree of hardneſs, though this be the ſtate beſt adapted to the retention of magnetiſm. You will yourſelf readily diſcern the defects of this method by *the ſimple touch*.

Let us ſuppoſe that B is the pole of the loadſtone from which the magnetic matter iſſues, as the effect of the two poles is ſo ſimilar that it is impoſſible to perceive the ſlighteſt difference: having reſted the pole on the extremity *b* of the needle, the magnetic matter enters into it with all the rapidity with which it moves in the loadſtone, incomparably greater than that of the vortex which is in the external air. But what will become of this matter in the needle? It cannot get out at the extremity *b*, it will, therefore, make an effort to force it's way through the needle toward *a*, and the pole B moving in the ſame direction, will aſſiſt this effort; but as ſoon as the pole B ſhall arrive at *a*, the difficulty of eſcaping at the extremity *a* will occaſion a contrary effort, by which the magnetic matter will be impelled from *a* toward *b*; and before the firſt effect is entirely deſtroyed, this laſt cannot take place. Afterwards, when the pole B is again brought back to the extremity *b*, this laſt effect is again deſtroyed, but without producing, however, a current in the contrary direction from *b* toward *a*; and conſequently, when the pole B ſhall have got beyond *c* in it's progreſs toward *a*, it will more eaſily produce a current from *a* to *b*, eſpecially if you preſs more hard on the half *c a* : hence it is clear, that the needle can have acquired only a ſmall degree of the magnetic power.

Some, accordingly, rub only the half *c a (plate III. fig.* 10.) proceeding from *c* to *a*, and others touch only the extremity *a* of the needle, with the pole B of the loadſtone, and with nearly the ſame ſucceſs. But it is evident that the magnetic matter which enters by the extremity *a* only, is incapable of acting with ſufficient vigor on the pores of the needle, for arranging them conformably to the laws of magnetiſm; and that the force impreſſed by this method muſt be extremely ſmall, if any thing, when the ſteel is very much hardened.

It appears to me, then, that theſe defects of *the ſimple touch* might be remedied in the following manner; of the ſucceſs of which I entertain no doubt, though I have not yet tried it, but am confirmed in my opinion by experiments which I have made.

I would caſe the extremity *b* of the needle, *(plate III. fig.* 11.) in a ruler of ſoft iron E F; and I ſhould think it proper to make that ruler very thin, and as ſtraight as poſſible, but the extremity muſt be exactly applied in all points, and even fitted to a groove perfectly adjuſted for it's reception. On reſting the pole B of the loadſtone upon the extremity *b* of the needle, the magnetic matter which enters into it, meeting ſcarcely any difficulty in it's progreſs through the iron ruler, will at once purſue it's courſe in the direction [illegible] in proportion as the pole advances toward [illegible] tic matter, in order to continue this courſe, has only to arrange the pores on which it immediately acts; and having reached *a*, all theſe pores, or at leaſt by far the greater part of them, will be already diſpoſed conformably to that direction. When you afterwards re-commence the friction at the extremity *b*, nothing is deſtroyed, but you continue to perfect the current of the magnetic matter, following the ſame direction *b d* by likewiſe arranging the pores which reſiſted the firſt operation, and thus the magnetic canals, in the needle, will always become more perfect. A few ſtrokes of the pole B will be ſufficient for the purpoſe, provided the loadſtone is not too weak: and I have no doubt, that the beſt tempered ſteel, that is, rendered as hard as poſſible, would yield to this method of operating; an unſpeakable advantage in the conſtruction of compaſſes, as it has been found that ordinary needles frequently loſe, by a ſlight accident, all their magnetic power; by which ſhips at ſea would be expoſed to the greateſt dangers, if they had not others in reſerve. But when needles are made of well tempered ſteel, accidents of this kind are not ſo much to be apprehended; for if a greater force is requiſite to render them magnetic, in return they preſerve the power more tenaciouſly.

24th November, 1761.

Plate III

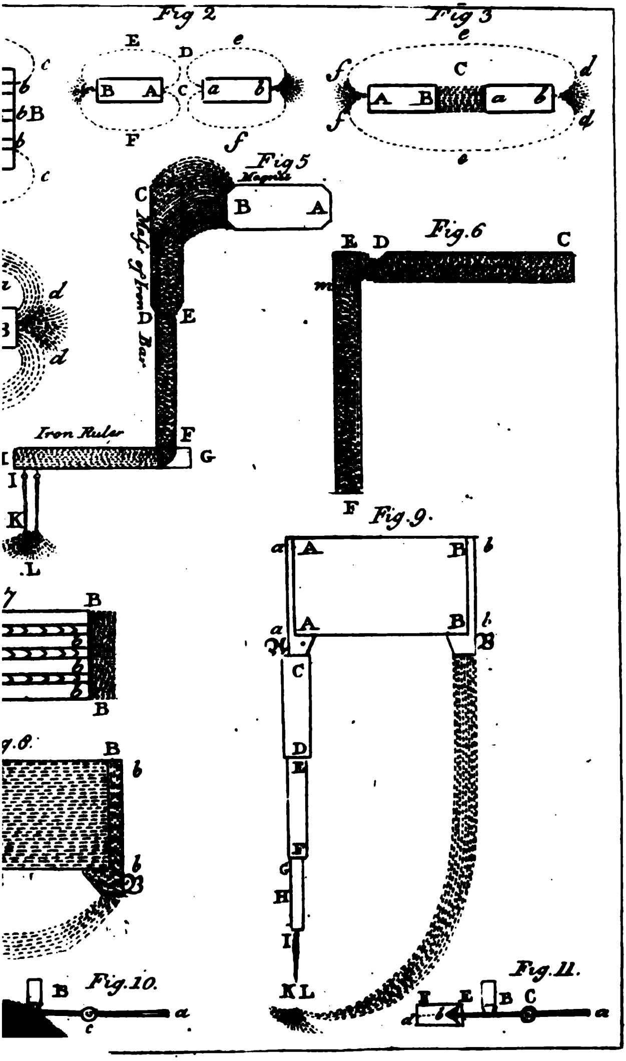


Fig 2
E
D
e
B
A
C
a
b
F
f
Fig 3
e
C
f
d
A
B
a
b
e
c
b
bB
b
c
Fig 5
Magnet
B
A
C
Mass of Iron Bar
D
E
F
G
Iron Ruler
I
K
L
d
d
Fig. 6
E
D
C
m
F
Fig. 9.
a
A
B
b
a
A
B
b
C
D
E
F
G
H
I
KL
7
B
b
b
b
B
g. 8.
B
b
b
Fig. 10.
B
c
a
Fig. 11.
E
E
B
C
d
b
a

LETTER LXIX.

On the Double Touch. *Means of preſerving the Magnetic Matter in magnetized Bars.*

INSTEAD of this method of magnetizing iron or ſteel by *the ſimple touch*, by rubbing with one pole only of the loadſtone, we now employ *the double touch*, in which we rub with both poles at once, which is eaſily done by means of an armed loadſtone.

Let E F, *(plate IV. fig.* 4.*)* be a bar of iron or ſteel which you wiſh to render magnetic. Having fixed it ſteadily on a table, you preſs upon it the two feet A and B of an armed loadſtone. In this ſtate, you will eaſily ſee that the magnetic matter, which iſſues from the loadſtone by the foot B muſt penetrate into the bar, and would diffuſe itſelf in all directions, did not the foot A, on it's ſide, attract the magnetic matter contained in the pores of the bar. This evacuation, therefore, at *d* will determine the matter, which enters by the pole B, to take it's courſe from *c* toward *d*. provided the poles A and B are not too remote from each other. Then the magnetic current will force it's way in the bar, in order to paſs from the pole B to the pole A, diſpoſing it's pores to form magnetic canals; and it is very eaſy to diſcover whether this is taking place; you have only to obſerv [illegible] adſtone is powerfully attracted to the bar, which never fails, if the bar is of ſoft iron, as the magnetic matter eaſily penetrates it. But if the bar is of ſteel, the attraction is frequently very ſmall, a proof that the magnetic matter is incapable of opening for itſelf a paſſage from *c* to *d*; hence it is to be concluded that the loadſtone is too feeble, or that the diſtance between it's two poles is too great: in this caſe it would be neceſſary to employ a loadſtone more powerful, or whoſe feet are nearer, or, finally, the armour of the loadſtone ought to be changed into the form repreſented in *plate IV. fig.* 3.

But here is a method for remedying this inconvenience.

Having diſpoſed, at ſmall intervals *c d (plate IV. fig.* 4.) the powers conformably to magnetiſm, the loadſtone muſt be ſeveral times drawn backward and forward over the bar, from one extremity to the other, without taking it off, till you perceive that the attraction no longer increaſes; for it is undoubtedly certain, that attraction is increaſed in proportion to the increaſe of the magnetic force. The bar E F will be magnetized by this operation, in ſuch a manner that the extremity E, toward which the pole A was turned, will be the friendly pole of A, and conſequently of the ſame name with the other pole B. Again, on removing the loadſtone, as magnetic canals are formed the whole length of the bar, the magnetic matter diffuſed through the air will force a paſſage through theſe canals, and will make the bar a real magnet. It will enter by the extremity *a* and eſcape by the extremity *b*, from whence a part, at leaſt, will return to *a*, and will form a vortex ſuch as the nature of the bar permits.

I take this occaſion to remark, that the formation of a vortex is abſolutely neceſſary to the increaſe of magnetiſm; for if all the magnetic matter which goes out at the extremity *b* were to fly off, and be entirely diſperſed, without returning to *a*, the air would not ſupply a ſufficient quantity to the other extremity *a*, which muſt occaſion a diminution of the magnetic force. But if a conſiderable part of that which eſcapes at the extremity *b* returns to *a*, the air is abundantly able to ſupply the remainder, and perhaps ſtill more, if the magnetic canals of the bar are capable of receiving it; the bar will therefore, in that caſe, acquire a much greater magnetic force.

This conſideration leads me to explain how it is poſſible to keep up the magnetic matter in magnetized bars. The object being to prevent the magnetic matter, which pervades them, from diſperſing in the air, theſe bars are always diſpoſed in *pairs* of exactly the ſame ſize. They are placed on a table, in a parallel ſituation, ſo that the friendly poles, or thoſe of different names, ſhould be turned to the ſame ſide as in *fig*. 5.

M M and N N repreſent the two bars, whoſe friendly poles *a b*, *b a* are turned the ſame way. To prevent miſtake, a mark × is made on each bar, at th[illegible]mity where the north pole is, and to both [illegible]ed a piece of ſoft iron E E and F F, for receiving the magnetic current. In this manner, the whole magnetic matter which pervades the bar M M, and which iſſues at the extremity *b*, paſſes into the piece of iron E E, where it eaſily makes it's way, to enter at the extremity *a* of the other bar N N, from which it will eſcape at the extremity *b* into the other piece of iron F F, which re-conveys it into the firſt bar M M by the extremity *a*. Thus the magnetic matter will continue to circulate, and no part of it eſcape; and even in caſe there ſhould not be at firſt a ſufficient quantity to ſupply the vortex, the air will ſupply the deficiency, and the vortex will preſerve all it's force in the two bars.

This diſpoſition of the two bars may likewiſe be employed for magnetizing both of them at once. The two poles of a loadſtone muſt be drawn over the two bars, paſſing from the one to the other by the pieces of iron, and the circuit muſt be ſeveral times performed, carefully obſerving that the two poles of the loadſtone A and B be turned as the figure directs.

This method of magnetizing two bars at once, muſt be much more efficacious than the preceding, as from the very firſt circuit performed by the loadſtone, the magnetic matter will begin to flow through the two bars by means of the two pieces of iron. Afterwards, by repeated circuitous applications of the loadſtone to the bars, a greater quantity of pores will be arranged in them conformably to magnetiſm, and more magnetic canals will be opened, by which the vortex will be more and more ſtrengthened, without undergoing any diminution. If the bars are thick, it would be proper to turn and rub them, in the ſame manner, on the other ſurfaces, in order that the magnetic action may penetrate them thoroughly.

Having obtained theſe magnetic bars M M, N N, (*plate IV. fig.* 6.) they may be employed, in place of the natural loadſtone, for magnetizing others. They are joined together a-top, ſo that the two friendly poles *a b* may touch each other; and the other two poles below, *b* and *a*, are ſeparated as far as it is thought proper. Then we rub with the two under extremities, which ſupply the place of the two poles of a loadſtone, two other bars E F in the manner which I have above explained.

As theſe two bars are joined in the form of compaſſes, we have the advantage of opening the lower extremities as much or as little as we pleaſe, which cannot be done with a loadſtone; and the magnetic current will eaſily paſs a-top, where the bars touch each other, from the one to the other. A ſmall piece of ſoft iron P might likewiſe be applied there, the better to keep up the current: and in this manner you may eaſily and ſpeedily magnetize as many double bars as you pleaſe.

28th November, 1761.

LETTER LXX.

The Method of communicating to Bars of Steel a very great Magnetic Force, by Means of other Bars which have it in a very inferior Degree.

THOUGH this method of magnetizing by the *double touch* be preferable to the preceding, the magnetic power, however, cannot be carried beyond a certain degree. Whether we employ a natural loadſtone, or two magnetic bars for rubbing other bars, theſe laſt will never acquire ſo much force as the firſt; it being impoſſible that the effect ſhould be greater than the cauſe.

If the bars with which we rub have little force, thoſe which are rubbed will have ſtill leſs: the reaſon is evident; for as bars deſtitute of magnetic force never could produce it in others, ſo a moderate degree of force is incapable of producing one greater than itſelf, at leaſt by the method which I have been deſcribing.

But this rule is not to be taken in the ſtrict interpretation of the words, as if it were literally impoſſible to produce a greater magnetic force by the aſſiſtance of a ſmaller. I am going to point out a method by which the magnetic power may be increaſed almoſt as far as you pleaſe, beginning with the ſmalleſt degree poſſible. This is a late diſcovery, which merits ſo much the more attention that it throws much light on a very difficult ſubject, the nature of magnetiſm.

Suppoſ

Suppoſing me poſſeſſed of a very feeble loadſtone, or, for want of a natural magnet, of bars of iron rendered ſomewhat magnetic, merely by the vortex of the earth, as I explained it in a preceding letter, I then provide myſelf with eight bars of ſteel very ſmall and not hardened, in order the more eaſily to receive the ſmall degree of magnetic power which the feeble loadſtone, or ſlightly magnetized bars, are capable of communicating, by rubbing each pair or couple in the manner I formerly deſcribed. Having then eight bars, magnetic but in a very ſmall degree, I take two pair, which I join together in the manner repreſented, *plate IV. fig.* 7.

By uniting the two bars by the poles of the ſame name, I form but one of double the thickneſs, and with which I form the compaſs A C and B D; the better to keep up the magnetic current, a piece of ſoft iron P may be applied at the top C D. The legs of the compaſs may be ſeparated as far as is judged proper, and I rub with them, one after the other, the remaining bars, which will thereby acquire more power than they had before, becauſe the powers of the firſt are now united. I have now only to join theſe two pair newly rubbed, in the ſame manner, and by rubbing with them one after the other, the two pair firſt employed, and the power of theſe will be conſiderably increaſed. I afterwards join theſe two pair to[illegible]and go on rubbing others in order to augmen[illegible]etic force, and ſtill two pair with two [illegible]; and by repeating this operation th[illegible] may be carried to [illegible] a degree, as to become inſuſceptible of farther increaſe, even by continuing the operation. When we have more than four pair of ſuch bars, inſtead of two pair, three may be joined together for the purpoſe of rubbing others; they will thereby be ſooner carried to the higheſt degree poſſible.

The greateſt obſtacles are, therefore, ſurmounted, and, by means of ſuch bars joined together by two or more pairs, we may rub others of ſteel properly hardened, and which may be either of the ſame ſize, or ſtill greater than the firſt, to which the greateſt power of which they are ſuſceptible may be thus communicated.

Beginning with ſmall bars ſuch as I have deſcribed, theſe operations may be ſucceſſively applied to bars of an enormous ſize, and made of the hardeſt ſteel which is leſs liable to loſe the magnetic power. Only it is to be obſerved, that for the purpoſe of rubbing large bars, ſeveral pairs ought to be joined together, whoſe united weight ſhould be at leaſt double that of the large one. But it would always be better to proceed by degrees, and to rub each ſpecies of bars with bars not much ſmaller than themſelves, or it may be ſufficient to join at moſt two pair: for when we are obliged to join more than two pair, the extremities with which the friction is performed will extend too far, and the magnetic matter, which paſſes that way, will itſelf prevent it's being directed conformably to the direction of the bar that is rubbed; and the rather, that it enters the bar perpendicularly, whereas it neceſſarily ſhould take a horizontal direction.

In order to facilitate this change of direction, it is proper that the magnetic matter ſhould be led to it in a ſmall ſpace, and in a direction already approaching to that which it ought to take within the bar which we are going to rub. The following method, I think, might be effectual for this purpoſe.

Plate IV. fig. 8. repreſents five pair of bars M M, N N, joined together, but not in the form of a compaſs. There is at top a bar of ſoft iron C D, to keep up the vortex; below, I do not rub immediately with the extremities of the bars, but I caſe theſe extremities on each ſide in a foot of ſoft iron, faſtening them to it with ſcrews marked O. Each foot is bent at A and B, ſo that the direction of the magnetic matter, which freely pervades theſe feet, already has a conſiderable approximation to the horizontal, ſo that in the bar to be rubbed E F it has no need greatly to change it's direction. I have no doubt that, by means of theſe feet, the bar E F will receive a much greater magnetic power, than if we rubbed immediately by the extremities of the bars, the depth of whoſe vertical direction naturally oppoſes the formation of horizontal magnetic canals in the bar E F. It is likewiſe poſſible, in practiſing this method, to contract or extend the diſtance of the feet A and B, at pleaſure.

I muſt farther obſerve, that when theſe bars loſe, in time, their magnetic power, it is eaſily reſtored by the ſame operation.

1ſt *December*, 1761.

LETTER

LETTER LXXI.

[illegible] Magnets in Form of a Horſe Shoe.

[illegible] wiſhes to make experiments on [illegible] of the loadſtone, ought to be [illegible] great number of magnetic bars, [illegible] up to a very large ſize. Each [illegible] a particular magnet, having its [illegible] and the other ſouth.

[illegible] conſidered it as extremely remar[illegible] [illegible] of the magnetic power, [illegible] be ſupplied by a wretched na[illegible] [illegible] a pair of tongs in the chimney [illegible] acquired by length of time a [illegible] magnetiſm, we ſhould be enabled to [illegible] to ſuch a degree, as to commu[illegible] [illegible] of ſteel, the higheſt degree [illegible] which they are ſuſceptible. It [illegible] ſaid that, by this method, we [illegible] the beſt magnetic needles, [illegible] than the common, but made [illegible] the higheſt degree, which ren[illegible] I have only a few words [illegible] artificial magnets, which [illegible] a horſe-ſhoe, as you muſt

[illegible] the ſame purpoſes, [illegible] natural, with this advantage [illegible] much more powerful, by giving them a ſufficient magnitude. They are made of well-tempéred ſteel, and the figure of a horſe-ſhoe ſeems the moſt proper for keeping up the vortex. When the mechanic has finiſhed his work, we communicate to it the greateſt degree of magnetic power of which it is ſuſceptible, by means of the magnetic bars of which I have given a deſcription. It is evident that the greater this magnet is, the larger muſt be the bars we employ; and this is the reaſon why we ſhould be provided with bars of all ſizes.

In order to magnetize a horſe-ſhoe H I G, (*plate IV. fig.* 9.) which ought to be of ſteel well tempered, we place on the table a pair of magnetic bars A C and B D, with their ſupporters of ſoft iron applied on both ſides, but of which the figure repreſents only one E F, the other having been removed to make way gradually for the application of the feet of the horſe-ſhoe, as you ſee. In this ſtate, the magnetic matter, which pervades the bars, will make ſtrong efforts to paſs through the horſe-ſhoe, the poles of the bars being adapted magnetically to thoſe of the horſe-ſhoe; but conſidering the hardneſs of tempered ſteel, it will not be ſufficient to arrange the pores and open for itſelf a paſſage. The ſame means, therefore, muſt be employed to this effect, which were preſcribed for the magnetizing of bars. We take a compaſs formed of another pair of magnetic bars, and rub them in the ſame manner over the horſe-ſhoe; magnetic canals will thereby be opened, and the ſubtile matter of the bars, by pervading it, will form the vortex of that fluid. Particular care muſt be taken, in this operation, that the legs of the compaſs, in paſſing over the horſe-ſhoe, do not touch the extremities A and B of the bars; for this would diſturb the current of the magnetic matter, which would paſs immediately from the bars into the legs of the compaſs; or, the vortices of the bars and of the compaſs would mutually derange each other.

The horſe-ſhoe will thereby acquire very great power, being pervaded by an impetuous magnetic current. All that remains to be done, is to detach the bars, without deranging the current. If they are ſeparated violently, the magnetic vortex will be deſtroyed, and the artificial magnet will retain very little power.

The canals being kept up no longer than the magnetic matter pervades them, it muſt be concluded, that the particles which form theſe canals are in a forced ſtate, and that this ſtate ſubſiſts only while the vortex acts; and that as ſoon as it ceaſes, theſe particles, by their elaſticity, will deviate from their forced ſituation, and the magnetic canals will be interrupted and deſtroyed. This we clearly ſee in the caſe of ſoft iron, whoſe pores are quickly arranged, on the approach of a magnetic vortex, but retain ſcarcely any magnetic power, when removed out of the vortex. This proves that the pores of iron are moveable, but endowed with an elaſticity which changes their ſituation, as ſoon as force ceaſes. It requires length of time to fix certain pores in the poſition impreſſed on them by the magnetic force, which takes place chiefly in bars of iron long expoſed to the vortex of the earth. The pores of ſteel are much leſs flexible, and better ſupport the ſtate into which they have been forced; they are, however, liable to ſome derangement, as ſoon as force ceaſes to act on them, but this derangement is leſs in proportion to the hardneſs of the ſteel. For this reaſon, artificial magnets ought to be made of the hardeſt ſteel: were they to be made of iron, they would immediately acquire, on being applied to magnetic bars, a very great degree of power; but the moment you detach them, all that power would diſappear. Great precaution muſt, therefore, be employed, in ſeparating from the bars magnets compoſed of well-tempered ſteel. For this purpoſe, before the ſeparation, you preſs the ſupporter, which is of very ſoft iron, in the direction of the line M N, (*plate IV. fig.* 10.) taking particular care not to touch the bars with it, for this would mar the whole proceſs, and oblige you to repeat the operation. On the application of the ſupporter, a conſiderable portion of the magnetic matter which is circulating in the magnet G H I, will make its way through the ſupporter, and form a ſeparate vortex, which will continue after the magnet is detached from the bars.

Afterwards you preſs the ſupporter ſlowly forward over the legs of the magnet to the extremities, as repreſented in the figure, and in this ſtate permit it to reſt for ſome time, that the vortex may be allowed to ſettle. The ſupporter is likewiſe furniſhed with a weight P, which may be increaſed every day; it being always underſtood, that the ſupporter is to be ſo perfectly adjuſted to the feet of the magnet, as to touch them in all points.

5th December, 1761.

LETTER LXXI.

On Dioptricks; Inſtruments which that Science ſupplies: of Teleſcopes and Microſcopes. Different Figures given to Glaſſes or Lenſes.

THE wonders of dioptricks will now, I think furniſh a ſubject worthy of your attention This ſcience provides us with two kinds of inſtruments compoſed of glaſs, which ſerve to extend our ſphere of viſion, by diſcovering objects which would eſcape the naked eye.

There are two caſes in which the eye needs aſſiſtance: the firſt is, when objects are too diſtant to admit of our ſeeing them diſtinctly; ſuch are the heavenly bodies, reſpecting which the moſt important diſcoveries have been made by means of dioptrical inſtruments. You will pleaſe to recollect what I have ſaid, concerning the ſatellites of Jupiter, which aſſiſt us in the diſcovery of the longitude: they are viſible only with the aid of good teleſcopes, and thoſe of Saturn require teleſcopes of a ſtill better conſtruction.

There are, beſides, on the ſurface of the earth objects very diſtant, which it is impoſſible for us to ſee, and to examine in detail, without the aſſiſtance of teleſcopes, which repreſent them to us in the ſame manner, as if they were near. Theſe dioptrical glaſſes or inſtruments, for viewing diſtant bodies, are denominated by the term we have already employed, *teleſcopes* or perſpectives.

The other caſe, in which the eye needs aſſiſtance, is when the object, though ſufficiently near, is too ſmall to admit of a diſtinct examination of it's parts. If we wiſhed, for example, to diſcover all the parts of the leg of a fly, or of any inſect ſtill ſmaller: if we were diſpoſed to examine the minuter particles of the human body, ſuch as the ſmalleſt fibres of the muſcles, of the nerves, it would be impoſſible to ſucceed without the help of certain inſtruments called *microſcopes*, which repreſent ſmall objects in the ſame manner as if they were a hundred or a thouſand times greater.

Here then are two kinds of inſtruments, teleſcopes and microſcopes, furniſhed by dioptricks for aſſiſting the weakneſs of our ſight. A few ages only have elapſed ſince theſe inſtruments were invented; and from the era of that invention muſt be dated the moſt important diſcoveries in aſtronomy, by means of the teleſcope, and in phyſics by the microſcope.

Theſe wonderful effects are produced merely by the figure given to bits of glaſs, and the happy combination of two or more glaſſes, which we denominate *lenſes*. Dioptricks is the ſcience that unfolds the principles on whi [illegible] ruments are conſtructed, and the uſes to [illegible] applied, and you will pleaſe to recollect that it turns chiefly on the direction which rays of light take on paſſing through tranſparent mediums of a different quality; on paſſing, for example, from air into glaſs or water, and reciprocally from glaſs or water into air.

As long as the rays are propagated through the ſame medium, ſay air, they preſerve the ſame direction, in the ſtraight lines L A, L B, L C, L D, (*plate IV. fig.* 2.) drawn [illegible] the luminous point L, whence theſe rays proceed, and when they any where meet an eye, as at ſea, they enter into it, and there paint an image of the object from which they proceeded. In this caſe the viſion is denominated ſimple, or natural; and repreſents to us the objects as they really are. The ſcience, which explains to us the principles of this viſion, is termed *optics*.

But when the rays, before they enter into the eye, are reflected on a finely poliſhed ſurface, ſuch as a mirror, the viſion is no longer natural; as in this caſe we ſee the objects differently, and in a different place, from what they really are. The ſcience which explains the phenomena preſented to us by this viſion from reflected rays, is termed *catoptricks*. It to ſupplies us with inſtruments calculated to extend th ſphere of our viſion, and you are acquainted wit ſuch ſorts of inſtruments, which, by means of on or two mirrors, render us the ſame ſervices as thoſ conſtructed with lenſes. Theſe are what we properl denominate *teleſcopes*: but in order to diſtinguiſ them from the common perſpectives, which are compoſed only of glaſſes, it would be better to call t optrick reflecting teleſcopes. This mode of ex- ſſion would at leaſt be more accurate; for the rd teleſcope was in uſe before the diſcovery of re- ting inſtruments, and then meant the ſame thing h perſpective.

propoſe, at preſent, to confine myſelf entirely dioptrical inſtruments, of which we have two ts, teleſcopes or perſpectives, and microſcopes. the conſtruction of both we employ glaſſes formed er different manners, the various ſorts of which m going to explain. They are principally three, ording to the figure given to the ſurface of the ſs.

The firſt is the *plane*, when the ſurface of a glaſs is ne on both ſides, as that of a common mirror. If u were to take, for example, a piece of looking- ſs, and to ſeparate from it the quick-ſilver which heres to it's farther ſurface, you would have a glaſs th of whoſe ſurfaces are *plane*, and of the ſame ickneſs throughout.

The ſecond is the *convex*: a glaſs of this denomi- tion is more raiſed in the middle than toward the ge.

The third is the *concave*: ſuch a glaſs is hollow ward the middle, and riſes toward the edge.

Of theſe three different figures, which may be ven to the ſurface of a glaſs, are produced the ſix cies of glaſſes repreſented in *fig.* 12.

I. The *plano-plane* glaſs [illegible] h it's ſurfaces ne.

II. The *plano-convex* glaſs has one ſurface plane and the other convex.

III. The *plano-concave* has one ſurface plane and the other concave.

IV. The *convexo-convex*, or *double convex*, has both ſurfaces convex.

V. The *convexo-concave*, or *meniſcus*, has one ſurface convex and the other concave.

VI. Finally, the *concavo-concave*, or *double concave*, has both ſurfaces concave.

It is proper to remark that the figure repreſents the cut of theſe glaſſes or lenſes.

8th December, 1761.

LETTER LXXIII.

Difference of Lenſes with reſpect to the Curve of their Surfaces. Diſtribution of Lenſes into Three Claſſes.

FROM what I have ſaid reſpecting the convex and concave ſurfaces of lenſes, you will eaſily comprehend that their form may be varied without end, according as the convexity and concavity are greater or leſs. There is only one ſpecies of plane ſurfaces; becauſe a ſurface can be plane in one manner only; but a convex ſurface may be conſidered as making part of a ſphere, and, according as the radius or diameter of that ſphere is greater or leſs, the convexity will differ; and as we repreſent lenſes on paper by ſegments of a circle, according as theſe circles are greater or leſs, the form of lenſes mu

Fig. 1.

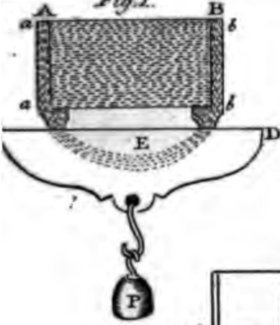


Fig. 2

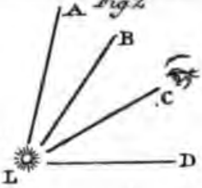


Fig. 3

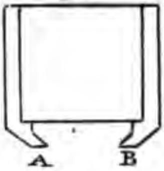


Fig. 4

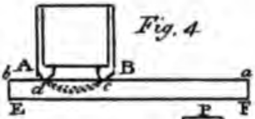


Fig. 5

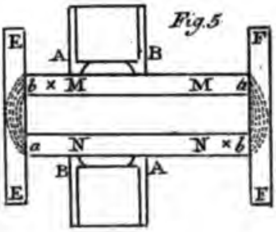


Fig 6

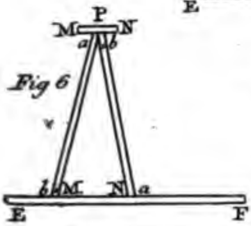


Fig. 7.

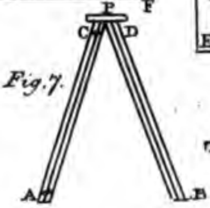


Fig. 8.

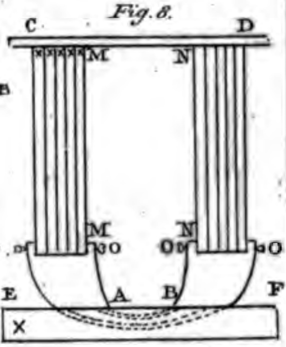


Fig. 9.

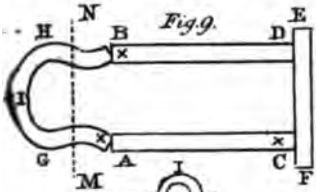


Fig. 10

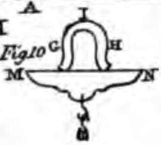


Fig. 11

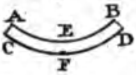
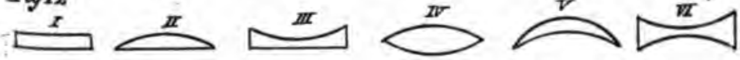


Fig. 12



or leſs, I ſhall here preſent you with ſeveral figures of different convexity; (ſee *plate V. fig.* 1.)

From this you ſee, that the ſmaller the radius is, the greater is the curve of the ſurface, or the greater it's difference from the plane; on the contrary, the greater the radius is, the more the ſurface approaches to a plane, or the arch of the circle to a ſtraight line. If the radius were made ſtill greater, the curve would at length become hardly perceptible. You ſcarcely perceive it in the arch MN, (*fig.* 2.) the radius of which is ſix inches, or half a foot; and if the radius were ſtill extended to ten or a hundred times the magnitude, the curve would become altogether imperceptible to the eye.

But this is by no means the caſe as to dioptricks; and I ſhall afterwards demonſtrate, that though the radius were a hundred or a thouſand feet, and the curve of the lens abſolutely imperceptible, the effect would neverthelefs be abundantly apparent. The radius muſt indeed be inconceivably great, to produce a ſurface perfectly plane: from which you may conclude, that a plane ſurface might be conſidered as a convex ſurface whoſe radius is infinitely great, or as a concave of a radius infinitely great. Here it is that convexity and concavity are confounded, ſo that the plane ſurface is the medium which ſeparates convexity from concavity. But the ſmaller the radii are, the greater and more perceptible do the convexities and concavities become; and hence, we ſay reciprocally, that a convexity or concavity is greater in proportion as it's radius, which is the meaſure of it, is ſmaller.

However great, in other reſpects, may be the variety we meet with in lenſes or glaſſes, according as their ſurfaces are plane, convex, or concave, and this in an infinity of different manners; nevertheleſs, with reſpect to the effect reſulting from them in dioptricks, they may be reduced to the three following claſſes:

The firſt comprehends glaſſes which are every where of an equal thickneſs; whether their two ſurfaces be plane and parallel to each other, (*fig.* 3.) or the one convex and the other concave, but concentric, or deſcribed round the ſame centre (*fig.* 4.) ſo that the thickneſs ſhall remain every where the ſame. It is to be remarked reſpecting glaſſes of this claſs, that they produce no change in the appearance of the objects which we view through them; the objects appear exactly the ſame as if nothing interpoſed; accordingly, they are of no manner of uſe in dioptricks. This is not becauſe the rays which enter into theſe glaſſes undergo no refraction, but becauſe the refraction at the entrance is perfectly ſtraightened on going off, ſo that the rays, after having paſſed through the glaſs, reſume the ſame direction which they had purſued before they reached it. Glaſſes, therefore, of the other two claſſes, on account of the effect which they produce, conſtitute the principal object of dioptricks.

The ſecond claſs of lenſes contains

thicker at the middle than at the

Their effect is the ſame, as long as the exceſs of the thickneſs of the middle over that of the edge has the ſame relation to the magnitude of the lens. All lenſes of this claſs are commonly denominated *convex*, as convexity predominates, though otherways one of their ſurfaces may be plain, and even concave.

The third claſs contains all thoſe lenſes which are thicker at the edge than in the middle (*plate V. fig.* 6.) which all produce a ſimilar effect, depending on the exceſs of thickneſs toward the edge, over that in the middle. As concavity prevails in all ſuch lenſes, they are ſimply denominated *concave*. They muſt be carefully diſtinguiſhed from thoſe of the ſecond claſs, which are the convex.

Lenſes of theſe two laſt claſſes are to be the ſubject of my following letters, in which I ſhall endeavour to explain their effects in dioptricks.

12th December, 1761.

LETTER LXXIV.

Effect of Convex Lenſes.

IN order to explain the effect produced by both convex and concave lenſes, in the appearance of objects, two caſes muſt be diſtinguiſhed, the one when the object is very far diſtant from the lens, and the other when it is nearer.

But before I enter on the explanation of this, I muſt ſay a few words on what is called the axis of the lens. As the two ſurfaces are repreſented by ſegments of a circle, you have only to draw a ſtraight line through the centres of the two circles; this line is named the axis of the lens. In *fig. 7. plate V.* the centre of the arch A E B being at C, and that of the arch A F B at D, the ſtraight line C D is denominated the axis of the lens A B, and it is eaſy to ſee that this axis paſſes through the middle of it. The ſame thing would apply, if the ſurfaces of the lens were concave. But, if one is plane, the axis will be perpendicular to it, paſſing through the centre of the other ſurface.

Hence it is obvious, that the axis paſſes through the two ſurfaces perpendicularly, and that accordingly, a ray of light coming in the direction of the axis, will ſuffer no refraction, becauſe rays paſſing from one medium into another are not broken or refracted, except when they do not enter in a perpendicular direction.

It may likewiſe be proved that all other rays paſſing through the middle of the lens O, undergo no refraction, or rather that they again become parallel to themſelves.

It muſt be conſidered, in order to comprehend the reaſon of this, that at the points E and F the two ſurfaces of the lens are parallel to each other, for the angle M E B, which the ray M E makes with the arch of the circle E B, or it's tangent at E, is perfectly equal to the angle P F A, which this ſame ray produced, F P, makes with the arch of the circle A F, or it's tangent at F; you recollect that two ſuch angles are denominated alternate, and that it is demonſtrated, when the alternate angles are equal, that the ſtraight lines are parallel to each other: conſequently the two tangents at E and at F will be paparallel, and it will be the ſame thing as if the ray M E F P paſſed through a lens whoſe two ſurfaces were parallel to each other. Now, we have already ſeen that rays do not change their direction in paſſing through ſuch a lens.

Having made theſe remarks, let us now conſider a convex lens A B (*plate V. fig.* 8.) whoſe axis ſhall be the ſtraight line O E F P, and let us ſuppoſe that there is in this line, at a great diſtance from the lens, an object or luminous point O, which diffuſes rays in all directions; ſome of theſe will paſs through our lens A B, ſuch as O M, O E, and O N; of which that in the middle O E will undergo no refraction, but will continue it's direction through the lens in the ſame produced ſtraight line F I P. The other two rays O M and O N, in paſſing through the lens toward the edge, will be refracted, both at entering and departing, ſo that they will ſomewhere meet the axis, ſay at I, and afterwards continue their progreſs in the directions I Q and I R. It might likewiſe be demonſtrated that all the rays which fall between M and N will be refracted, ſo as to meet with the axis in the ſame point I. Therefore the rays which, had no lens interpoſed, would have purſued their rectilineal direction O M and O N, will, after the refraction, purſue other directions, as if they had taken their departure from the point I; and if there were an eye ſomewhere at P, it would be affected juſt as if the luminous point were actually at I, though there be no reality in this. You have only to ſuppoſe for a moment, that there is at I a real object, which, diffuſing it's rays, would be equally ſeen by an eye placed at P, as it now ſees the object at O by means of the rays refracted by the lens, becauſe there is at I an image of the object O, and the lens A B there repreſents the object O, or tranſports it nearly to I. The point O is therefore no longer the object of viſion, but rather it's image, repreſented at I; for this is now it's immediate object.

This lens, then, produces a very conſiderable change: an object very remote O is ſuddenly tranſported to I, from which the eye muſt undoubtedly receive a very different impreſſion from what it would do, if, withdrawing the lens, it were to view the object O immediately. Let O be conſidered as a ſtar, the point O being ſuppoſed extremely diſtant, the lens will repreſent at I the image of that ſtar, but an image which it is impoſſible to touch, and which has no reality, as nothing exiſts at I, unleſs it be that the rays proceeding from the point O are collected there by the refraction of the lens. Neither is it to be imagined, that the ſtar would appear to us in the ſame manner as if it really exiſted at I. How could a body, many thouſands of times bigger than the earth, exiſt at a point I? Our ſenſes would be very differently ſtruck by it: We muſt carefully remark, then, that [illegible] only is repreſented at I, like that of

LETTE

The ſame Subject: Diſtanc
Lenſe

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directions, we confine our attention to thoſe which fall on the lens.

My remarks ſhall be at preſent farther limited to the rays iſſuing from the point O, ſituated in the very axis of the lens. The figure repreſents three of theſe rays, O A, O M and O N, the firſt of which, O A, paſſing through the middle of the lens, undergoes no change of direction, but proceeds, after having paſſed through the lens, in the ſame ſtraight line B I S, that is in the axis of the lens; but the other two rays, O M and O N, undergo a refraction both on entering into the glaſs, and leaving it, by which they are turned aſide from their firſt direction, ſo as to meet ſomewhere at I with the axis, from which they will proceed in their new direction, in the ſtraight lines M I Q and N I R; ſo that afterwards, when they ſhall meet an eye, they will produce in it the ſame effect as if the point O exiſted at I, as they preſerve the ſame direction. For this reaſon, the convex lens is ſaid to tranſport the object O to I; but in order to diſtinguiſh this point I from the real point O, the former is called the image of the latter, which in it's turn is denominated the object.

This point I is very remarkable, and when the object O is extremely diſtant, the image of it is likewiſe denominated the focus of the lens, of which I ſhall explain the reaſon. If the ſun be the object at O, the rays which fall on the lens are all collected at I, and being endowed with the quality of heating, it is natural that the concourſe of ſo many rays at I, ſhould produce a degree of heat, capable of ſetting on fire any combuſtible matter that may be placed there. Now, the place where ſo much heat is collected we call the *focus*; the reaſon of this denomination with reſpect to convex lenſes is evident. Hence, too, a convex lens is denominated a *burning-glaſs*, the effects of which you are undoubtedly well acquainted with. I only remark that this property of collecting the rays of the ſun, in a certain point called their focus, is common to all convex lenſes; they likewiſe collect the rays of the moon, of the ſtars, and of all very diſtant bodies; though their force is too ſmall to produce any heat, we nevertheleſs employ the ſame term, focus; the focus of a glaſs, accordingly, is nothing elſe but the ſpot where the image of very diſtant objects is repreſented: to which this condition muſt ſtill be added, that the object ought to be ſituated in the very axis of the lens; for if it be out of the axis, it's image will likewiſe be repreſented out of the axis: I ſhall have occaſion to ſpeak of this afterward.

It may be proper, ſtill farther, to ſubjoin the following remarks reſpecting the focus:

1. As the point O, or the object, is infinitely diſtant, the rays O M, O A, and O N, may be conſidered as parallel to each other, and, for the ſame reaſon, parallel to the axis of the lens.

2. The focus I, therefore, is the point behind the glaſs, where the rays parallel to the axis, which falls on the lens, are collected by the refraction of the lens.

3. The focus of a lens, and the ſpot where the

image of an object, infinitely diſtant, and ſituated in the axis of the lens, is repreſented, are the ſame thing.

4. The diſtance of the point I behind the lens, that is the length of the line B I, is called the diſtance of the focus of the lens. Some authors call it the focal diſtance.

5. Every convex lens has it's particular diſtance of focus, one greater, another leſs, which is eaſily aſcertained by expoſing the lens to the ſun, and obſerving where the rays meet.

6. Lenſes formed by arches of ſmall circles, have their focuſes very near behind them; but thoſe whoſe ſurfaces are arches of great circles, have more diſtant focuſes.

7. It is of importance to know the focal diſtance of every convex lens employed in dioptricks; and it is ſufficient to know the focus in order to form a judgment of all the effects to be expected from it, whether in the conſtruction of teleſcopes or microſcopes.

8. If we employ lenſes equally convex on both ſides, ſo that each ſurface ſhall correſpond to the ſame circle; then the radius of that circle gives nearly the focal diſtance of that lens: thus, to make a burning-glaſs which ſhall burn at the diſtance of a foot, you have only to form the two ſurfaces arches of a circle whoſe radius is one foot.

9. But when the lens is plano-convex, it's focal diſtance is nearly equal to the diameter of the circle, which correſponds to the convex ſurface.

Acquaintance with theſe terms will facilitate the knowledge of what I have farther to advance on this ſubject.

19th December, 1761.

LETTER LXXVI.

Diſtance of the Image of Objects.

HAVING remarked that an object infinitely diſtant, is repreſented by a convex lens in the very focus, provided ſuch object be in the axis of the lens, I proceed to nearer objects, but always ſituated in the axis of the glaſs; and I obſerve, firſt, that the nearer the object approaches to the lens, the farther the image retires.

Let us accordingly ſuppoſe that F (*plate V. fig.* 10.) is the focus of the lens M M, ſo that an object infinitely diſtant before the glaſs, or at the top of the figure, the image ſhall be repreſented at F; on bringing the object nearer to the glaſs, and placing it ſucceſſively at P Q R, the image will be repreſented at the point *p q r*, more diſtant from the lens than the focus: in other words, if A P is the diſtance of the object, B *p* will be the diſtance of the image, and if A Q is the diſtance of the object, B *q* will be that of the image, and the diſtance B *r* of the image will correſpond to the diſtance A R of the object.

There is a rule by which it is eaſy to calculate the diſtance of the image behind the lens, for every diſtance of the object before it, but I will not tire you with a dry expoſition of this rule; it will be ſufficient to remark, in general, that the more the diſtance of the object before the glaſs is diminiſhed, the more is the diſtance of the image behind it increaſed. I ſhall to this ſubjoin the inſtance of a convex lens, whoſe focal diſtance is 6 inches, or of a lens ſo formed, that if the diſtance of the object is infinitely great, the diſtance of the image behind the lens ſhall be preciſely ſix inches; now, on bringing the object nearer to the lens, the image will retire, according to the gradations marked in the following table:

Diſtance of the Object.	Diſtance of the Image.
Infinity	6
42	7
24	8
18	9
15	10
12	12
10	15
9	18
8	24
7	42
6	Infinity.

Thus the object being 42 inches diſtant from the lens, the image will fall at the diſtance of 7 inches, that is one inch beyond the focus. If the object is at the diſtance of 24 inches, the image will be removed to the diſtance of 8 inches from the lens, that is two inches beyc [illegible] us, and ſo of the reſt.

Though theſe numbers are applicable only to a lens, whoſe focal diſtance is 6 inches, ſome general conſequences may, however, be deduced from them.

1. If the diſtance of the object is infinitely great, the image falls exactly in the focus.

2. If the diſtance of the object is double the diſtance of the focus, the diſtance of the image will likewiſe be double the diſtance of the focus; in other words, the object and the image will be equally diſtant from the lens. In the example above exhibited, the diſtance of the object being 12 inches, that of the image is likewiſe 12 inches.

3. When the object is brought ſo near the lens, that the diſtance is preciſely equal to that of the focus, ſay 6 inches, as in the preceding example, then the image retires to an infinite diſtance behind the lens.

4. It is likewiſe obſervable in general, that the diſtance of the object and that of the image reciprocally correſpond, or, if you put the object in the place of the image, it will fall in the place of the object.

5. If, therefore, the lens MM *(plate V. fig. 11.)* collects at I the rays which iſſue from the point O, that ſame lens will likewiſe there collect rays iſſuing from the point I.

6. It is the conſequence of a great principle in dioptricks, in virtue of which it may be maintained, that whatever are the refractions which rays have undergone in paſſing through ſeveral refringent mediums, they may always return in the ſame direction.

This truth is of much importance in the knowledge of lenſes: thus when I know, for example, that a lens has repreſented, at the diſtance of 8 inches, the image of an object 24 inches diſtant, I may confidently infer, that if the object were 8 inches diſtant, the ſame lens would repreſent it's image at the diſtance of 24 inches.

It is farther eſſential to remark, that when the diſtance of the object is equal to that of the focus, the image will ſuddenly retire to an infinite diſtance; which perfectly harmonizes with the relation exiſting between the object and the image.

You will no doubt be curious to know in what place the image will be repreſented when the object is brought ſtill nearer to the lens, ſo that it's diſtance ſhall become leſs than that of the focus. This queſtion is the more embarraſſing, that the anſwer muſt be, the diſtance of the image will, in this caſe, be greater than infinity, ſince the nearer the object approaches the lens, the farther does the image retire. But the image being already infinitely diſtant, how is it poſſible that diſtance ſhould be increaſed? The queſtion might undoubtedly puzzle philoſophers, but is of eaſy ſolution to the mathematician. The image will paſs from an infinite diſtance, to the other ſide of the lens, and conſequently will be on the ſame ſide with the object. However ſtrange this anſwer may appear, it is confirmed, not only by reaſoning, but by experience, ſo that it is impoſſible to doubt of it's ſolidity; to increaſe beyond infinity is the ſame thing with paſſing to the other ſide: this is unqueſtionably a real par

LETTER LXXVII.

Magnitude of Images.

YOU can no longer doubt that every convex lens muſt repreſent ſomewhere the image of an object preſented to it; and that, in every caſe, the place of the image varies as much, according to the diſtance of the object, as according to the focal diſtance of the lens: but a very important article remains yet to be explained, I mean the magnitude of the image.

When ſuch a lens repreſents to us the image of the ſun, of the moon, or of a ſtar, at the diſtance of a foot, you are abundantly ſenſible that theſe images muſt be incomparably ſmaller than the objects themſelves. A ſtar being much greater than the whole earth, how is it poſſible that an image of ſuch magnitude ſhould be repreſented to us at the diſtance of a foot? But the ſtar appearing to us only as a point, the image, repreſented by the lens, likewiſe reſembles a point, and, conſequently, is infinitely ſmaller than the object itſelf.

There are then, in every repreſentation made by lenſes, two things to be conſidered; the one reſpects the place where the image is repreſented, and the other, the real magnitude of the image, which may be very different from that of the object. The firſt being ſufficiently elucidated, I proceed to furniſh you with a very ſimple rule, by which you will be enabled, in every caſe, to determine what muſt be the magnitude of the image repreſented by the lens.

Plate V.

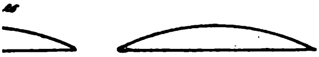
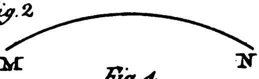


Fig. 2



ch

Fig 3

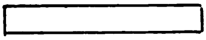
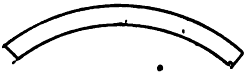
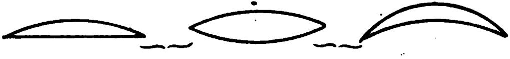


Fig 4



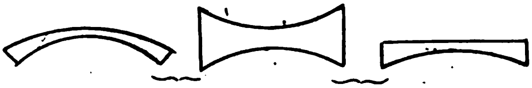
Inch

Fig 5



Inch

Fig. 6



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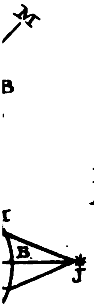


Fig. 8

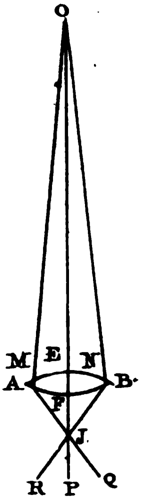


Fig. 9

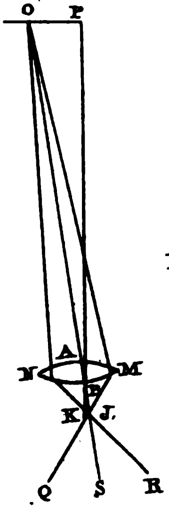
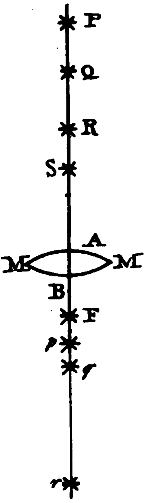


Fig 10



Let O P *(plate VI. fig.* 1.*)* be any object whatever, ſituated on the axis of the convex lens M N; we muſt firſt look for the place of the image, which is at I, ſo that the point I ſhall be the repreſentation of the extremity O of the object, as the rays iſſuing from the point O are there collected by the refraction of the lens. Let us now ſee in what place will be repreſented the other extremity P of the object; for this purpoſe, let us conſider the rays P M, P A, P N, which, iſſuing from the point P, fall on the lens; I obſerve that the ray P A, which paſſes through the middle of the lens, does not change it's direction, but continues it's progreſs in the ſtraight line A K S; it will be, therefore, ſomewhere in this line at K, that the other rays P M and P N will meet: in other words, the point K will be the image of the other extremity P of the object, the point I being that of the extremity O: hence it is eaſy to conclude that I K will be the image of the object O P, repreſented by the lens.

In order then, to determine the magnitude of this image, having found the place I, you have only to draw from the extremity P of the object, through A, the middle of the lens, the ſtraight line P A K S, and to raiſe from I the line I K perpendicular to the axis, and this line I K will be the image in queſtion; it is evident from this that the image is reverſed, ſo that if the line O R were horizontal, and the object O P a man, the image would have the head K undermoſt, and the feet I upward.

On this I ſubjoin the following remarks:

1. The nearer the image (*plate VI. fig. 2.*) is to the lens, the ſmaller it is; and, the more remote it is, the greater it's magnitude. Thus O P being the object placed on the axis before the lens M N, if the image fell at Q it would be ſmaller than if it fell at R, S, or T. For, as the ſtraight line P A *t*, drawn from the ſummit of the object P, through the middle of the lens, always terminates the image at whatever diſtance it may be, it is evident, that among the lines Q *q*, R *r*, S *s*, T *t*, the firſt Q *q* is the ſmalleſt, and that the others increaſe in proportion as they remove from the lens.

2. There is one caſe in which the image is preciſely equal to the object: it is when the diſtance of the image is equal to that of the object; and this takes place, as I have already remarked, when the diſtance of the object A O is double that of the focus of the lens; the image will then be T *t*, ſo that the diſtance B *t* is equal to A O. You have only then to conſider the two triangles O A P and T A *t*, which having the oppoſite angles at the point A, as well as the ſides A O and A T equal each to each, as likewiſe the angles at O and T, which are both right angles: theſe two triangles will be every way equal, and conſequently the ſide T *t*, which is the image, will be equal to the ſide O P, which is the object.

3. If the image were twice farther from the lens than the object, it would be double the object; and, in general, as many times as the image is farther from the lens than the object, ſo many times will it be greater than the object. For the nearer you bring

the object to the glaſs, the farther the image retires, and conſequently the greater it becomes.

4. The contrary takes place when the image is nearer the lens than the object; it is then as many times ſmaller than the object, as it is nearer the lens than the object is. If then the diſtance of the image were one thouſand times leſs than that of the object, it would likewiſe be one thouſand times ſmaller.

5. Let us apply this to burning-glaſſes, which, being expoſed to the ſun, repreſent it's image in the focus, or rather repreſent the focus, that is, the luminous and brilliant circle which burns, and which is nothing elſe but the image of the ſun repreſented by the lens. You will no longer be ſurprized, then, at the ſmallneſs of the image, notwithſtanding the prodigious magnitude of the ſun, it being as many times ſmaller in the focus than the real ſun, as the diſtance of the ſun from the lens is greater than that of the image.

6. Hence likewiſe it is evident, that the greater is the diſtance of the focus of a burning-glaſs, the more brilliant alſo is the circle in the focus, that is, the greater will be the image of the ſun: and the diameter of the focus is always about one hundred times ſmaller than the diſtance of the focus from the lens.

I ſhall afterwards explain the different uſes which may be made of convex lenſes; they are all ſufficiently curious to merit attention.

26th December, 1761.

LETTER LXXVIII.

Burning Glaſſes.

THE firſt uſe of convex lenſes, is their employment as burning-glaſſes, the effect of which muſt appear altogether aſtoniſhing, even to thoſe who already have ſome acquaintance with natural philoſophy. In fact, who could believe, that the image of the ſun, ſimply, ſhould be capable of exciting ſuch a prodigious degree of heat? But your ſurpriſe will ceaſe, if you pleaſe to pay ſome attention to the following reflections.

1. Let M N, (*plate VI. fig.* 3.) be a burning-glaſs, which receives on it's ſurface the rays of the ſun R, R, R, refracted in ſuch a manner as to preſent at F a ſmall luminous circle, which is the image of the ſun, and ſo much ſmaller as it is nearer to the glaſs.

2. All the rays of the ſun, which fall on the ſurface of the glaſs, are collected in the ſmall ſpace of the focus F; their effect, accordingly, muſt, in that ſpace, be as many times greater as the ſurface of the glaſs exceeds the magnitude of the focus, or of the ſun's image. We ſay that the rays, which were diſperſed over the whole ſurface of the glaſs, are concentrated in the ſmall ſpace F.

3. The rays of the ſun having a certain degree of heat, they exert their power, in a very ſenſible manner, at the focus; it is poſſible even to calculate how many times the heat at the focus muſt exceed the

natural heat of the ſun's rays: we have only to obſerve how many times the ſurface of the glaſs is greater than the focus.

4. If the glaſs were not greater than the focus, the heat would not be ſtronger at the focus than any where elſe; hence we muſt conclude, that in order to the production of a ſtrong heat by a burning-glaſs, it is not ſufficient that it ſhould be convex, or that it ſhould repreſent the image of the ſun, it muſt, beſides, have a ſurface which ſeveral times exceeds the magnitude of the focus, which is ſmaller in proportion as it is nearer to the glaſs.

5. France is in poſſeſſion of the moſt excellent burning-glaſs; it is three feet in diameter, and it's ſurface is calculated to be nearly two thouſand times greater than the focus, or the image of the ſun which it repreſents. It muſt produce, therefore, in the focus, a heat two thouſand times greater than that which we feel from the ſun. It's effects are, accordingly, prodigious: wood of every kind is, in a moment, ſet on fire; metals are melted in a few minutes; and, in general, the moſt ardent fire which we are capable of producing, is not once to be compared with the vehement heat of this focus.

6. The heat of boiling water is calculated to be about thrice greater than what we feel from the rays of the ſun in ſummer, or, which amounts to the ſame thing, the heat of boiling water is thrice greater than the natural heat of the blood in the human body. in order to melt lead, we muſt have a heat thrice than is requiſite to make water boil; and to

melt copper a heat ſtill thrice greater is neceſſary. To melt gold requires a much higher degree of heat. Heat, then, one hundred times greater than that of our blood is capable of melting gold; how far then muſt a heat two thouſand times greater exceed the force of our ordinary fires?

7. But how are theſe prodigious effects produced by the rays of the ſun collected in the focus of a burning-glaſs? This is a very difficult queſtion, with reſpect to which philoſophers are very much divided. Thoſe who maintain that the rays are an emanation from the ſun, darted with the amazing velocity which I formerly deſcribed, are not greatly embarraſſed for a ſolution; they have only to ſay that the matter of the rays, ſtriking bodies with violence, muſt totally break and deſtroy their minute particles. But this opinion is no longer admitted in ſound philoſophy.

8. The other ſyſtem, which makes the nature of light to conſiſt in the agitation of the ether, appears little adapted to explain theſe ſurprizing effects of burning-glaſſes. On carefully examining, however, all the circumſtances, we ſhall ſoon be convinced of the poſſibility of this. The natural rays of the ſun, as they fall on bodies, excite the minute particles of the ſurface to a concuſſion, or motion of vibration, which, in it's turn, is capable of exciting new rays, and by theſe the body in queſtion is rendered viſible. And a body is illuminated only ſo far as theſe proper particles are put into a motion of vibration ſo rapid as to be capable of producing new rays in the ether.

9. It is clear, then, that if the natural rays of the

ſun have ſufficient force to agitate the minute particles of bodies, thoſe which are collected in the focus muſt put the particles which they meet there into an agitation ſo violent, that their mutual adheſion is entirely diſſolved, and the body itſelf completely deſtroyed, which is the effect of fire. For if the body is combuſtible, as wood, the diſſolution of theſe minute particles, joined to the moſt rapid agitation, makes a conſiderable part of it to fly off into air, in the form of ſmoke, and the groſſer particles remain in form of aſhes. Fuſible bodies, as metals, become liquid by the diſſolution of their particles, whence we may comprehend how fire acts on bodies; it is only the adheſion of their minuteſt particles which is attacked, and the particles themſelves are thereby afterwards put into the moſt violent agitation. Here, then, is a very ſtriking effect of burning-glaſſes, which derives its origin from the nature of convex lenſes. There are beſides many other wonderful effects to be deſcribed.

28th December, 1761.

LETTER LXXIX.

The Camera Obſcura.

WE likewiſe employ convex lenſes in the *camera obſcura*, and by means of them, all external objects are preſented in the darkened room on a white 'n their natural colours, in ſuch a manner s and public buildings, or objects in general, are repreſented in much greater perfection than the power of the pencil is capable of producing. Painters accordingly avail themſelves of this method, in order to draw, with exactneſs, landſcapes and other objects which are viewed at a diſtance. The camera obſcura, then, is to be the ſubject of this letter.

E F G H, (*plate VI. fig. 4.*) repreſents the ſquare of a camera obſcura, cloſely ſhut up on all ſides, except one little round aperture made in one of the window ſhutters, in which is fixed a convex lens, of ſuch a focus as to throw the image of external objects, ſay the tree O P, exactly on the oppoſite wall F G, at $p\,q$. A white and moveable table is likewiſe employed, which is put in the place of the images repreſented.

The rays of light, therefore, can be admitted into the chamber only through the aperture M N, in which the lens is fixed, without which total darkneſs would prevail.

Let us now conſider the point P of any object, ſay the ſtem of our tree O P. It's rays P M, P A, P N will fall, then, on the lens M N, and be refracted by it, ſo as to meet again at the point p on the wall, or on a white table placed there for the purpoſe. This point p will conſequently receive no other rays but ſuch as proceed from the point P; and in like manner every other point of the table will receive only the rays which proceed from the correſponding point of the object; and reciprocally, to every point of the external object will correſpond a point on the table, which receives thoſe rays and no other. If the lens were to be removed from the aperture MN, the table would be illuminated in quite a different manner, for in that caſe every point of the object would diffuſe it's rays over the whole table, ſo that every point of the table would be illuminated at once by all the external objects, whereas at preſent it is ſo by one only, that whoſe rays it receives; from this you will eaſily comprehend that the effect muſt be quite different from what it would be if the rays entered ſimply by the aperture MN into the chamber.

Let us now examine, ſomewhat more cloſely, wherein this difference conſiſts; and let us firſt ſuppoſe that the point P of the object is green, the point of the table *p* will, therefore, receive only thoſe green rays of the object P, and theſe, re-uniting on the wall or table, will make a certain impreſſion, which here merits conſideration. For this purpoſe you will pleaſe to recollect the following propoſitions which I had formerly the honour of explaining to you.

1. Colours differ from each other in the ſame manner as muſical ſounds: each colour is produced by a determinate number of vibrations, which, in a given time, are excited in the ether. The green colour of our point P is accordingly appropriated to a certain number of vibrations, and would no longer be green were theſe vibrations more or leſs rapid. Though we do not know the number of vibrations which produce ſuch or ſuch a colour, we may, however, be permitted to ſuppoſe here that green [illegible] twelve thouſand vibrations in a ſecond; [illegible] affirm of this nu[illegible] twelve

and,

thouſand, may likewiſe be eaſily underſtood of the real number, whatever it be.

2. This being laid down, the point *p* on the white table will be ſtruck by a motion of vibration, of which twelve thouſand will be completed in a ſecond. Now, I have remarked that the particles of a white ſurface are all of ſuch a nature as to receive every ſort of agitation, more or leſs rapid, whereas thoſe of a coloured ſurface are adapted to receive only that degree of rapidity which correſponds to their colour. And as our table is white, the point *p* in it will be excited to a motion of vibration correſponding to the colour of green; in other words it will be agitated twelve thouſand times in a ſecond.

3. As long as the point *p*, or the particle of the white ſurface which exiſts there, is agitated with a ſimilar motion, this will be communicated to the particles of the ether which ſurround it; and this motion diffuſing itſelf in all directions, will generate rays of the ſame nature, that is to ſay, green; juſt as in muſic, the ſound of a certain note, ſay C, agitates a ſtring wound up to the ſame tone, and makes it emit a ſound without being touched.

4. The point *p* of the white table will accordingly produce green rays, as if it were dyed or painted that colour: and what I affirm of the point *p*, will equally take place with reſpect to all the points of the illuminated table, which will produce all the rays, each of the ſame colour with that of the object whoſe image it repreſents. Every point of the table will,

therefore, become viſible, under a certain colour, as if it were actually painted that colour.

5. You will perceive, then, on the table, all the colours of the external objects, the rays of which will be admitted into the chamber through the lens: each point in particular will appear of the colour of that point of the object which correſponds to it, and you will ſee on the table a combination of various colours, diſpoſed in the ſame order as you ſee them in the objects themſelves, that is to ſay, a repreſentation, or rather the perfect picture of all the objects on the outſide of the dark chamber which are before the lens NN.

6. All theſe objects will, however, appear reverſed on the table, as you will conclude from what I have ſaid in my foregoing letters. The under part of the tree O will be repreſented at *o*, and the ſummit P at *p*: for, in general, each object muſt be repreſented on the white table, in the place which is the termination of the ſtraight line drawn from the object P through the middle of the lens A: that which is upward will, conſequently, be repreſented downward, and that which is to the left will be to the right; in a word, every thing will be reverſed in the picture; the repreſentation will, nevertheleſs, be more exact and more perfect than the moſt accurate painter is capable of producing.

7. You will further remark, that this picture will be ſo much ſmaller than the objects themſelves, in proportion as the focus of the lens is ſhorter. Lenſes of a ſhort focus will accordingly give the objects in

miniature; and if you would wiſh to have them magnified, you muſt employ lenſes of a longer focus, or which repreſent the images at a greater diſtance.

8. In order to contemplate theſe repreſentations more at eaſe, the rays may be intercepted by a mirror, from which they are refracted, ſo as to repreſent the whole picture on a horizontal table; and this is of peculiar advantage when we wiſh to copy the images.

2d January, 1762.

LETTER LXXX.

Reflections on the Repreſentation in the Camera Obſcura.

THOUGH you can no longer entertain any doubt reſpecting the repreſentations made in a dark chamber, by means of a convex lens, I hope the following reflections will not appear ſuperfluous, as they ſerve to place this ſubject in a clearer light.

1. The chamber muſt be completely darkened, for, were the light admitted, the white table would be viſible, and the particles of it's ſurface, already agitated, would be incapable of receiving the impreſſion of the rays which unite to form the images of external objects. Though, however, the chamber were a little illuminated, ſtill ſomething of the repreſentation would appear on the table, but by no means ſo vivid as if the chamber were entirely dark.

2. We muſt carefully diſtinguiſh the picture repreſented on the white table, from the image which

the lens, in virtue of it's own nature, repreſents, as I have formerly explained. It is very true, that placing the table in the very place where the image of the objects is formed by the lens, this image will be confounded by the picture we perceive on the table; theſe two things are, nevertheleſs, of a nature entirely different: the image is only a ſpectre or ſhade floating in the air, which is viſible but in certain places, whereas the repreſentation is a real picture, which every one in the chamber may ſee, and to which duration alone is wanting.

3. In order the more clearly to elucidate this difference, you have only to conſider carefully the nature of the image *o*, (*plate VI. fig.* 5.) repreſented by the convex lens MN, the object being at O. This image is nothing elſe but the place in which the rays OM, OC, ON of the object, after having paſſed through the lens, meet by refraction, and thence continue their direction as if they proceeded from the point *o*, though they really originated from O, and by no means from *o*.

4. Hence the image is viſible only to eyes ſituated ſomewhere within the angle R*o*Q, as at S, where an eye will actually receive the rays which come to it from the point *o*. But an eye ſituated out of this angle, as at F or V, will ſee nothing at all of it, becauſe no one of the rays collected at *o* is directed toward it: the image at *o*, therefore, differs very eſſentially from a real object, and is viſible only in certain places.

5. But if a white table is placed at *o*, and it's ſurface at this point *o* is really excited to an agitation ſimilar to that which takes place in the object O, this ſpot *o* of the ſurface itſelf generates rays which render it viſible every where. Here, then, is the difference between the image of an object, and it's repreſentation made in a camera obſcura: the image is viſible only in certain places, namely, thoſe through which are tranſmitted the rays that originally proceed from the object; whereas the picture, or repreſentation formed on the white table, is ſeen by it's own rays, excited by the agitation of the particles of it's ſurface, and conſequently viſible in every place of the camera obſcura.

6. It is likewiſe evident, that the white table muſt abſolutely be placed exactly in the place of the image formed by the lens, in order that every point of the table may receive no other rays except ſuch as proceed from a ſingle point of the object: for if other rays were likewiſe to fall upon it, they would diſturb the effect of the former, or render the repreſentation confuſed.

7. Were the lens to be entirely removed, and free admiſſion given to the rays into the dark chamber, the white table would be illuminated by it, but no picture would be viſible. The rays of the different objects would fall on every point of the table, without expreſſing any one determinate image. The picture, accordingly, which we ſee in a camera obſcura, on a white ſurface, is the effect of the convex

lens fixed in the ſhutter: this it is which collects anew, in a ſingle point, all the rays that proceed from one point of the object.

8. A very ſingular phenomenon is here, however, obſervable, when the aperture, made in the window-ſhutter of the dark chamber, is very ſmall: for though no lens be applied, you may, nevertheleſs, perceive on the oppoſite partition the images of external objects, and even with their natural colours: but the repreſentation is very faint and confuſed, and if the aperture is enlarged this repreſentation entirely diſappears. I ſhall explain this phenomenon.

In *fig.* 6. *plate VI.* MN is the ſmall aperture through which the rays of external objects are admitted into the dark chamber EFGH. The wall FG oppoſite to the aperture is white, the better to receive the impreſſion of rays of all ſorts.

Let the point O be an object, of which the rays OM, ON alone, with thoſe which fall between them, can enter into the chamber. Theſe rays will be confined to the ſmall ſpace *o o* of the wall, and will illuminate it. This ſpace *o o* will be ſo much ſmaller, or approach the nearer to a point, in proportion as the aperture MN is ſmall: if then this aperture were very ſmall, we ſhould have the effect already deſcribed, according to which every point of the white table receives only the rays proceeding from a ſingle point of the object: there would be produced, of conſequence, a repreſentation ſimilar to that which is prod [illegible] lication of a convex lens to an [illegible] hutter. But in t caſe, the aperture being of a certain extent, every point O of the object will illuminate a certain ſmall ſpace *o o* on the wall, and agitate it by it's rays. The ſame thing then, nearly, would take place, as if a painter, inſtead of making points with a fine pencil, ſhould with a coarſe one make ſpots of a certain magnitude, attending, however, to deſign and colouring, the repreſentation made on the wall will have a reſemblance to this ſort of daubing; but it will be clearer in proportion to the ſmallneſs of the aperture by which the rays are admitted.

5th January, 1762.

LETTER LXXXI.

Of the Magic Lantern, and Solar Microſcope.

THE camera obſcura has properly no effect except on very diſtant objects, but you will eaſily comprehend, that it's application may be equally extended to nearer objects; for this purpoſe the white table muſt be removed further from the lens, conformably to this general rule, that the nearer the object is brought to the convex lens, the farther does the image, where the white table ought to be placed, retire from it; and if the chamber is not of ſufficient depth, a different lens, of a ſhorter focus, muſt be employed.

You may place then, out of the chamber, before the aperture to which the convex lens is fitted, any object or picture whatever, and you will ſee a copy of it on the white table within the dark chamber, greater or ſmaller than the original, according as the diſtance of the image is greater or ſmaller; but it would be more commodious, undoubtedly, if the object could be expoſed within-ſide the dark chamber, in order to it's being moved and changed at pleaſure. But here a great difficulty occurs; the object itſelf would, in this caſe, be darkened, and conſequently rendered incapable of producing the effect we wiſh.

The thing wanted, then, is, to illuminate the object as much as poſſible, within-ſide the dark chamber, and at the ſame time to exclude the light. I have found out the means of doing this. You will recollect that I conſtructed a machine to the effect I am mentioning, which I had the honour of preſenting to you ſix years ago; and now you will eaſily comprehend the ſtructure, and the principles on which it is founded.

This machine conſiſts of a box very cloſe on all ſides, nearly of a figure ſimilar to *fig.* 7. *plate VII.* The farther ſide of which EG has an opening IK, in which are to be fitted the objects, portraits or other pictures OP which you mean to repreſent; on the other ſide, directly oppoſite, is a tube MNQR, containing a convex lens MN; this tube is moveable, for the purpoſe of bringing the lens nearer to the object, or of removing it at pleaſure. Then, provided the object OP be well illuminated, the lens will throw ſomewhere the image of it *o p*, and if [illegible] a white [illegible] you will ſee upon it [illegible] a perfect a perfect copy of the object, ſo much the clearer as the object itſelf is more illuminated.

For this purpoſe I have contrived in this box two ſide wings for the reception of lamps with large wicks, and in each wing is placed a mirror to reflect the light of the lamps on the objects OP; above, at EF, is a chimney by which the ſmoke of the lamps paſſes off. Such is the conſtruction of this machine, within which the object OP may be very ſtrongly illuminated, while the darkneſs of the chamber ſuffers no diminution. In order to the proper uſe of this machine, attention muſt be paid to the following remarks.

I. On ſliding inward the tube MNQR, that is bringing the lens MN nearer to the object OP, the image *op* will retire; the white tablet muſt therefore be removed backward, to receive the image at the juſt diſtance; the image will thereby be likewiſe magnified, and you may go on to enlarge it at pleaſure by preſſing the lens MN nearer and nearer to the object OP.

II. On removing the lens from the object, the diſtance of the image will be diminiſhed: the white tablet muſt in this caſe be moved nearer to the lens, in order to have a clear and diſtinct repreſentation, but the image will be reduced.

III. It is obvious that the image will be always reverſed; but this inconveniency is eaſily remedied; you have only to reverſe the object OP itſelf, turning it upſide down, and the image will be repreſented upright on the white tablet.

IV. It is a farther general remark, that the more the image is magnified on the white tablet, the leſs luminous and diſtinct it will be; but on reducing the image, it is rendered more diſtinct and brilliant. The reaſon is plain, the light proceeds wholly from the illumination of the object; the greater that the ſpace is, over which it is diffuſed, the more it muſt be weakened, and the more contracted it is, the more brilliant.

V. Accordingly, the more you wiſh to magnify the repreſentation, the more you muſt ſtrengthen the illumination of the object, by increaſing the light of the lamps in the wings of the machine: but for ſmall repreſentations a moderate illumination is ſufficient.

The machine which I have been deſcribing is called the *magic-lantern*, to diſtinguiſh it from the common camera obſcura, employed for repreſenting diſtant objects: its figure, undoubtedly, has procured it the name of lantern, eſpecially as it is deſigned to contain light; but the epithet *magic* muſt have been an invention of it's firſt proprietors, who wiſhed to impreſs the vulgar with the idea of magic or witchcraft. The ordinary magic-lanterns, however, are not conſtructed in this manner, and ſerve to repreſent no other objects but figures painted on glaſs, whereas this machine may be applied to objects of all ſorts.

It may even be employed for repreſenting the ſmalleſt objects, and for magnifying the repreſenta-[illegible] a prodigious ſiz[illegible] that the ſmalleſt fly ſhall

have occaſion to ſpeak of it
ſequel.

8th January, 1762.

LETTER LX

Uſe and Effect of a ſimple

WE likewiſe employ conve diately looking through explain their different uſes, we m inveſtigation of their nature.

Having obſerved the focal diſta I have already remarked, that v very remote, it's image is repreſ itſelf, but on bringing the object the image retires farther and farth if the diſtance of the object be e focus of the lens, the image is finite diſ as NF, NF; and as parallel lines are ſuppoſed to proceed forward to infinity, and as the image is always in the place where the rays, iſſuing from one point of the object, are collected again after the refraction; in the caſe when the object OA is equal to that of the focus of the lens, the place of the image removes to an infinite diſtance; and as it is indifferent whether we conceive the parallel lines NF and NF to meet at an infinite diſtance to the left or to the right, it may be ſaid indifferently, that the image is to the right or to the left, infinitely diſtant, the effect being always the ſame.

Having made this remark, you will eaſily judge what muſt be the place of the image, when the object is brought ſtill nearer to the lens.

Let OP, (*plate VI. fig.* 8.) be the object, and as it's diſtance OA from the convex lens is leſs than the diſtance of the focus, the rays OM, OM, which fall upon it from the point O, are too divergent to admit of the poſſibility of their being rendered parallel to each other by the refractive power of the lens; they will, therefore, be ſtill divergent after the refraction, as marked by the lines NF, NF, though much leſs ſo than before, therefore if theſe lines are produced backward, they will meet ſomewhere at *o*, as you may ſee in the dotted lines N *o*, N *o*. The rays NF, NF, will, of conſequence, after having paſſed through the lens, preſerve the ſame direction as if they had proceeded from the point *o*, though they have not actually paſſed through that point, as [illegible] in the lens that they have taken this new direction. An eye which receives theſe refracted rays NF, NF, will be, therefore, affected as if they really came from the point *o*, and will imagine that the object of it's viſion exiſts at *o*. There will, however, be no image at that point, as in the preceding caſe: to no purpoſe would you put a white tablet at *o*, it would preſent no picture there, for want of rays; for this reaſon we ſay that there is an imaginary image at *o*, and not a real one: the term *imaginary* being oppoſed to that of *real.*

Nevertheleſs, an eye placed at E receives the ſame impreſſion as if the object OP, from which the rays originally proceed, exiſted at *o*. It is of great importance, then, to know, as in the preceding caſes, the place and the magnitude of this imaginary image *op*. As to the place, it is ſufficient to remark, that if the diſtance of the object AO be equal to the diſtance of the focus of the lens, the image will be at an infinite diſtance from it, and this is what the preſent caſe has in common with the preceding; but the nearer the object is brought to the lens, or the leſs that the diſtance AO becomes than that of the focus of the lens, the nearer does the imaginary image approach to the lens, though, at the ſame time, it remains always at a greater diſtance from the lens than the object itſelf.

To elucidate this by an example, let us ſuppoſe that the focal diſtance of the lens is 6 inches, and for the different diſtances of the object, the anne[illegible] table indicates the diſtance of the imaginary im

Diſtance of the Object A O.	Diſtance of the imaginary Image A *o*.
6	Infinity
5	30
4	12
3	6
2	3
1	1 and a fifth.

The rule for aſcertaining the magnitude of this imaginary image *o p* is eaſy and general, you have only to draw through the middle of the lens, marked C, and through the extremity of the object P, the ſtraight line C P *p*; and where it meets with the line *o p* drawn from *o* at right angles with the axis of the lens, you will have found the magnitude of the imaginary image *o p*; from which it is evident, that this image is always greater than the object O P itſelf, as many times as it is farther from the lens than the object O P. It is likewiſe evident, that this image is not reverſed, as in the preceding caſe, but upright as the object.

You will eaſily comprehend, from what I have ſaid, the benefit that may be derived from lenſes of this ſort, by perſons whoſe ſight is not adapted to the view of near objects, but who can ſee them to more advantage at a conſiderable diſtance. They have only to look at objects through a convex lens, in order to ſee them as if they were very diſtant. The defect of ſight [illegible] reſpect to near objects occurs uſually in aged [illegible] who conſequently make uſe of ſpectacles with convex glaſſes, which, expoſed to the ſun, produce the effect of a burning-glaſs, and this aſcertains the focal diſtance of every glaſs. Some perſons have occaſion for ſpectacles of a very near focus, others of one more diſtant, according to the ſtate of their ſight; but it is ſufficient, for my preſent purpoſe, to have given a general idea of the uſe of ſuch ſpectacles.

12th January, 1762.

LETTER LXXXIII.

Uſe and Effect of a Concave Lens.

YOU have ſeen how convex glaſſes aſſiſt the ſight of old people, by repreſenting to them objects as at a greater diſtance than they really are: there are eyes, on the contrary, which, in order to diſtinct viſion, require the objects to be repreſented as nearer; and concave glaſſes procure them this advantage: which leads me to the explanation of the effect of concave lenſes, which is directly the contrary of that of the convex.

When the object O P, (*plate VI. fig.* 9.) is very diſtant, and it's rays O M, O M, fall almoſt parallel on the concave lens T V, in this caſe, inſtead of becoming convergent by the refraction of the lens, they, on the contrary, become more divergent, purſuing the direction N F, N F, which, produced backward, meet at the point *o*; ſo that an eye placed, for example, at E, receives theſe refracted rays

Fig 1

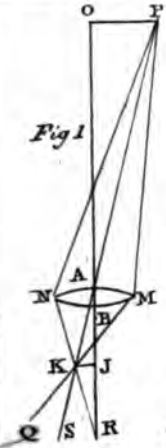


Fig. 2

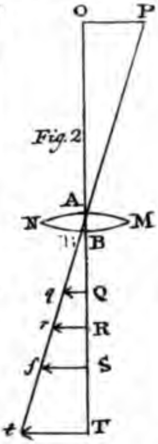


Fig 4

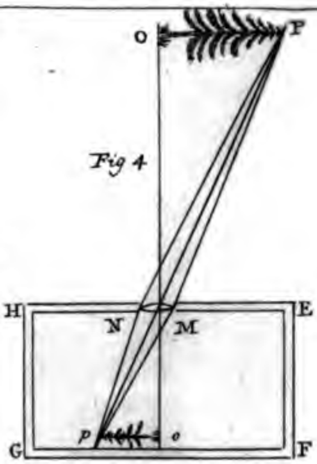


Fig 3

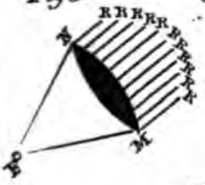


Fig. 5.

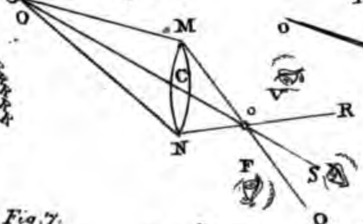


Fig 6

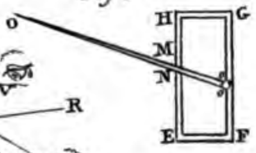


Fig. 7.

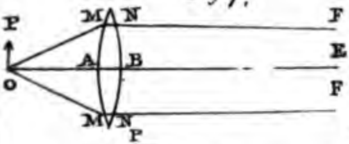


Fig. 8.

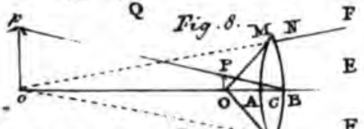
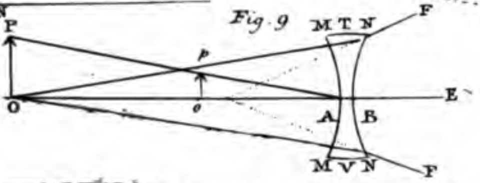


Fig. 9



ſame manner as if they proceeded from the point *o*, though they really proceed from the point O; for this reaſon I have, in the figure, dotted the ſtraight lines N *o*, N *o*.

As the object is ſuppoſed to be infinitely diſtant, were the lens convex the point *o* would be what we call the focus; but as, in the preſent caſe, there is no real concurrence of rays, we call this point, the imaginary focus of the concave lens; ſome authors likewiſe denominate it *the point of diſperſion*, becauſe the rays, refracted by the glaſs, appear to be diſperſed from this point.

Concave lenſes, then, have no real focus, like the convex, but only an imaginary focus, the diſtance of which from the lens A *o* is, however, denominated the focal diſtance of this lens, and ſerves, by means of a rule ſimilar to that which is laid down for convex lenſes, to determine the place of the image, when the object is not infinitely diſtant. Now this image is always imaginary, whereas in the caſe of convex lenſes, it becomes ſo only when the object is nearer than the diſtance of the focus. Without entering into the explication of this rule, which reſpects calculation merely, it is ſufficient to remark:

I. When the object O P is infinitely diſtant, the imaginary image *o p* is repreſented at the focal diſtance of the concave lens, and this too on the ſame ſide with the object. Nevertheleſs, though this image be imaginary, the eye placed at E is quite as much [illegible] as if it were real, conformably [illegible] on the ſubject of convex lenſes, when the object is nearer the lens than it's focal [illegible]ce.

II. [illegible]ringing the object O P nearer to the lens, it's i[illegible] *p* will likewiſe approach nearer, but in [illegible]ner, that the image will always be nearer t[illegible] lens than the object is; whereas, in the caſe of convex lenſes, the image is more diſtant from the lens than the object. In order to elucidate this more cl[illegible] [illegible]al diſtance of the concave [illegible]

I[illegible] O A	[illegible] Diſtance of the Image *o* A will be
[illegible]	[illegible]
3	2
2	1 and a half.

III. By the ſame rule you may always determine the magnitude of the imaginary image *o p*. You draw from the middle of the lens a ſtraight line, to the extremity of the object P, which will paſs through the extremity *p* of the image. For, ſince the line P A repreſents a ray coming from the extremity of the object, this ſame ray muſt, after the refraction, paſs through the extremity of the image; but, as this ray P A paſſes through the middle of the lens, it undergoes no refraction; therefore it muſt itſelf paſs through the extremity of the image, at the point *p*.

IV. This image is not reverſed, but in the ſame poſition with the object; and it may be laid down as a general rule, that whenever the image falls on the ſame ſide of the lens that the object is, it is always repreſented upright, whether the lens be convex or concave; but when repreſented on the other ſide of the lens, it is always reverſed; and this can take place only in convex lenſes.

V. It is evident, therefore, that the images repreſented by concave lenſes are always ſmaller than the objects; the reaſon is obvious, the image is always nearer than the object; you have only to look at the figure to be ſatisfied of this truth. Theſe are the principal properties to be remarked reſpecting the nature of concave lenſes, and the manner in which objects are repreſented by them.

It is now eaſy to comprehend how concave glaſſes may be rendered eſſentially ſerviceable to perſons whoſe ſight is ſhort. You are acquainted with ſome who can neither read nor write without bringing the paper almoſt cloſe to their noſe. In order, therefore, to their ſeeing diſtinctly, the object muſt be brought very near to the organ of viſion; I think I have formerly remarked that ſuch perſons are denominated *Miopes*. Concave lenſes, then, may be made of great uſe to them, for they repreſent the moſt diſtant objects as very near: the image not being farther from ſuch glaſſes than their focal diſtance, which, for the moſt part, is only a few inches.

Theſe images, it is true, are much ſmaller than the objects themſelves: but this by no means prevents diſtinctneſs of viſion. A ſmall object near, may appear greater than a very large body at a diſtance. In fact, a *two-dreyer* piece* appears to the eye greater than a ſtar in the heavens, though that ſtar far exceed the earth in magnitude.

Perſons whoſe ſight is ſhort, or *Miopes*, have occaſion, then, for glaſſes which repreſent objects as nearer; ſuch are concave lenſes. And thoſe whoſe ſight is long, or *Preſbites*, need convex glaſſes, which repreſent to them objects at a greater diſtance.

16th January, 1762.

LETTER LXXXIV.

Of apparent Magnitude, of the Viſual Angle, and of Microſcopes in general

I HAVE been remarking, that *Miopes* are obliged to make uſe of concave glaſſes to aſſiſt their viſion of diſtant objects, and that *Preſbites* employ convex glaſſes in order to a more diſtinct viſion of ſuch as are near: each ſight has a certain extent, and each requires a glaſs which ſhall repreſent objects perfectly. This diſtance in the *Miopes* is very ſmall, and in the *Preſbites* very great: but there are eyes ſo happily conformed, as to ſee nearer and more diſtant objects equally well.

Nevertheleſs, of whatever nature any perſon's ſight may be, this diſtance is never very ſmall: there is no

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* A ſmall ſilver coin, ſomewhat bigger than the pupil of the eye, in value the forty-eighth part of a crown.

Miope capable of ſeeing diſtinctly at the diſtance of leſs than an inch; you muſt have obſerved, that when the object is brought too cloſe to the eye, it has a very confuſed appearance; this depends on the ſtructure of the organ, which is ſuch in the human ſpecies, as not to admit of their ſeeing objects very near. To inſects, on the contrary, very diſtant objects are inviſible, while they eaſily ſee ſuch as are nearer. I do not believe that a fly is capable of ſeeing the ſtars, becauſe it can ſee extremely well at the diſtance of the tenth part of an inch, a diſtance at which the human eye can diſtinguiſh abſolutely nothing. This leads me to an explanation of the microſcope, which repreſents to us the ſmalleſt object as if it were very great.

In order to convey a juſt idea of it, I muſt entreat you carefully to diſtinguiſh between the apparent and the real magnitude of every object. Real magnitude conſtitutes the object of geometry, and is invariable as long as the body remains in the ſame ſtate. But apparent magnitude admits of infinite variety, though the body may remain always the ſame. The ſtars, accordingly, appear to us extremely ſmall, though their real magnitude is prodigious, becauſe we are at an immenſe diſtance from them. Were it poſſible to approach them, they would appear greater, from which you will conclude, that the apparent magnitude depends on the angle formed in our eyes, by the rays which proceed from the extremities of the object.

Let P O Q *(plate VII. fig.* 1.*)* be the object of viſion,

ſion, which, if the eye were placed at A, would appear under the angle P A Q, called the viſual angle, and which indicates to us the apparent magnitude of the object; it is evident, on inſpecting the figure, that the farther the eye withdraws from the object, the ſmaller this angle becomes, and that it is poſſible for the greateſt bodies to appear to us under a very ſmall viſual angle, provided our diſtance from them be very great, as is the caſe with the ſtars. But when the eye approaches nearer to the object, and looks at it from B, it will appear under the viſual angle P B Q, which is evidently greater than P A Q. Let the eye advance ſtill forward to C, and the viſual angle P C Q is ſtill greater. Farther, the eye being placed at D, the viſual angle will be P D Q; and on advancing forward to E, the viſual angle will be P E Q, always greater and greater. The nearer, therefore, the eye approaches to the object, the more the viſual angle increaſes, and conſequently likewiſe the apparent magnitude. However ſmall the object may be, it is poſſible, therefore, to increaſe it's apparent magnitude at pleaſure, you have only to bring it ſo near the eye as is neceſſary to form ſuch a viſual angle. A fly near enough to the eye may, of conſequence, appear under an angle as great as an elephant at the diſtance of ten feet. In a compariſon of this ſort, we muſt take into the account the diſtance at which we ſuppoſe the elephant to be viewed: unleſs this is done, we affirm abſolutely nothing; for an elephant appears great only when we are not very far from it; at the diſtance of a mile, it would be impoſſible,

mpoſſible, perhaps, to diſtinguiſh an elephant from a pig; and, tranſported to the moon, he would become abſolutely inviſible; and I might affirm with truth, that a fly appeared to me greater than an elephant, if the latter was removed to a very conſiderable diſtance. Accordingly, if we would expreſs ourſelves with preciſion, we muſt not ſpeak of the apparent magnitude of a body, without taking diſtance likewiſe into the account, as the ſame body may appear very great or very ſmall, according as it's diſtance is greater or leſs. It is very eaſy, then, to ſee the ſmalleſt bodies under very great viſual angles; they need only to be placed very cloſe to the eye.

This expedient may be well enough adapted to a fly, but the human eye could ſee nothing at too ſmall a diſtance, however ſhort it's ſight may be; beſides, perſons of the beſt ſight would wiſh to ſee likewiſe the ſmalleſt objects extremely magnified. The thing required, then, is to find the means of enabling us to view an object diſtinctly, notwithſtanding it's great proximity to the eye. Convex lenſes render us this ſervice, by removing the image of objects which are too near.

Let a very ſmall convex lens M N be employed, (*plate VII. fig.* 2.) the focal diſtance of which ſhall be half an inch; if you place before it a ſmall object O P, at a diſtance ſomewhat leſs than half an inch, the lens will repreſent the image of it *o p*, as far off as could be wiſhed. On placing the eye, then, behind the lens, the object will be ſeen as if it were at *o*, and at a ſufficient diſtance, as if it's magnitude

were

were *o p*: [illegible]s the eye is ſuppoſed very near the lens, the [illegible] angle will be *p t o*, that is the ſame as P *t* Θ, u[illegible]er which the naked eye would ſee the object O P [illegible] that proximity; but the viſion is become diſtinct by means of the lens: ſuch is the principle on which microſcopes are conſtructed.

19th January, 1762.

[illegible] R LXXXV.

Eſt[illegible] Objects viewed through [illegible]ope.

WHEN ſeveral perſons view the ſame object through a microſcope, the foot of a fly, for example, they all agree that they ſee it greatly magnified, but their judgment reſpecting the real magnitude will vary: one will ſay, it appears to him as large as that of a horſe; another, as that of a goat; a third, as that of a cat. No one, then, advances any thing poſitive on the ſubject, unleſs he adds, at what diſtance he views the feet of the horſe, the goat, or the cat. They all mean, therefore, without expreſſing it, a certain diſtance which is undoubtedly different; conſequently there is no reaſon to be ſurprized at the variety of the judgments which they pronounce, as the foot of a horſe, viewed at a diſtance, may very well appear no bigger than that of a cat, viewed near to the eye. Accordingly, when the queſtion is to be decided, How much does the microſcope magnify an object? we muſt accuſtom ourſelves to a more accurate mode of expreſſion, and particularly to ſpecify the diſtance, in the compariſon which we mean to inſtitute.

It is improper, therefore, to compare the appearances preſented to us by the microſcope, with objects of another nature, which we are accuſtomed to view ſometimes near, and ſometimes at a diſtance. The moſt certain method of regulating this eſtimation ſeems to be that which is actually employed by authors who treat of the microſcope. They compare a ſmall object viewed through the microſcope with the appearance which it would preſent to the naked eye, on being removed to a certain diſtance; and they have determined, that, in order to contemplate ſuch ſmall object to advantage by the naked eye, it ought to be placed at the diſtance of eight inches, which is the ſtandard for good eyes, for a ſhort-ſighted perſon would bring it cloſer to the eye, and one far-ſighted would remove it. But this difference does not affect the reaſoning, provided the regulating diſtance be ſettled; and no reaſon can be aſſigned for fixing on any other diſtance than that of eight inches, the diſtance received by all authors who have treated of the ſubject. Thus, when it is ſaid that a microſcope magnifies the object a hundred times, you are to underſtand that, with the aſſiſtance of ſuch microſcope, objects appear a hundred times greater than if viewed at the diſtance of eight inches, and thus you will form a juſt idea of the effect of a microſcope.

In general, a microſcope magnifies as many times as an object appears larger than if it were viewed underſtood that all the three dimenſions, ngth, breadth, and depth are repreſented greater. If then a microſcope ſhould 000 times, the whole extent of the object b ome 1000 times 1000 times 1000 times hich makes 1000,000,000, or a thouſand a moſt aſtoniſhing effect! This remark is eſſary to the formation of a juſt idea of what is r he power of microſcopes.

LXXXVI.

Fundam the Conſtruction of Simple Microſcopes. of ſome Simple Microſcopes.

HAVING explained in what manner we are enabled to judge of the power of microſcopes, it will be eaſy to unfold the fundamental principle for the conſtruction of ſimple microſcopes. And here it may be neceſſary to remark, that there are two kinds of microſcopes; ſome conſiſting of a ſingle lens, others of two or more, named, accordingly, ſimple or compound microſcopes, and which require particular elucidations. I ſhall confine myſelf, at preſent, to the ſimple microſcope, which conſiſts of a ſingle convex lens, the effect of which is determined by the following propoſition: *A ſimple microſcope magnifies as many times as it's focal diſtance is nearer than eight inches.* The demonſtration follows.

Let M N, *(plate VII. fig. 4.)* be a convex lens, whoſe focal diſtance, at which the object O P muſt be placed nearly, in order that the eye may ſee it diſtinctly, ſhall be C O; this object will be perceived under the angle O C P. But if it be viewed at the diſtance of eight inches, it would appear under an angle as many times ſmaller as the diſtance of eight inches ſurpaſſes the diſtance C O: the object will appear, therefore, as many times greater than if it were viewed at the diſtance of eight inches. Now, in conformity to the rule already eſtabliſhed, a microſcope magnifies as many times as it preſents the object greater than if we viewed it at the diſtance of eight inches. Conſequently a microſcope magnifies as many times as it's focal diſtance is leſs than eight inches. A lens, therefore, whoſe focal diſtance is an inch, will magnify preciſely eight times; and a lens whoſe focal diſtance is only half an inch, will magnify ſixteen times. The inch is divided into twelve parts, called *lines;* half an inch, accordingly, contains ſix lines; hence it would be eaſy to determine how many times every lens, whoſe focal diſtance is given in lines, muſt magnify; according to the following table:

Focal diſtance of the lens in lines.

	12.	8.	6.	4.	3.	2.	1.	½ lines
magnifies	8.	12.	16.	24.	32.	48.	96.	192 times

Thus a convex lens, whoſe focal diſtance is one line, magnifies ninety-ſix times, and if the diſtance be half a line, the microſcope will magnify one hundred and ninety-two, that is near two hundred times. Were greater effect ſtill to be deſired, lenſes muſt be conſtructed of a ſtill ſmaller focus. Now, it has been already remarked that, in order to conſtruct a lens of any certain given focus, it is only neceſſary to make the radius of each face equal to that focal diſtance, ſo that the lens may become equally convex on both ſides. I now proceed, then, to place before you (*plate VII. fig.* 5.) the form of ſome of theſe lenſes or microſcopes.

I. The focal diſtance of this lens A O is one inch or twelve lines. This microſcope, therefore, magnifies eight times.

II. The focal diſtance of the lens M N is eight lines. This microſcope magnifies twelve times.

III. The focal diſtance of the lens M N is ſix lines. This microſcope magnifies ſixteen times.

IV. The focal diſtance of this lens is four lines; and ſuch a microſcope magnifies twenty-four times.

V. The focal diſtance here is three lines. This microſcope magnifies thirty-two times.

VI. The focal diſtance here is two lines. This microſcope magnifies forty-eight times.

VII. The focal diſtance of this lens is only one line; and ſuch a microſcope magnifies ninety-ſix times.

It is poſſible to conſtruct microſcopes ſtill much ſmaller. They are actually executed, and much more conſiderable effects are produced; whence it muſt be carefully remarked, that the diſtance of the object from the glaſs becomes ſmaller and ſmaller, as it muſt be nearly equal to the focal diſtance of the lens. I ſay *nearly*, as every eye brings the glaſs cloſer to it, ſomewhat more or leſs, according to it's formation; the ſhort-ſighted apply it cloſer, the far-ſighted leſs ſo. You perceive then, that the effect is greater, as the microſcope or lens become ſmaller, and the cloſer likewiſe the object muſt be applied; this is a very great inconvenience, for, on the one hand, it is troubleſome to look through a glaſs ſo very ſmall, and, on the other, becauſe the object muſt be placed ſo near the eye. Attempts have been made to remedy this inconvenience, by a proper mounting, which may facilitate the uſe of it; but the viſion of the object is conſiderably diſturbed, as ſoon as the diſtance of it undergoes the ſlighteſt change: and as in the caſe of a very ſmall lens, the object muſt almoſt touch it, whenever the ſurface of the object is in the leaſt degree unequal, it is ſeen but confuſedly. For, while the eminences are viewed at the juſt diſtance, the cavities being too far removed, muſt be ſeen very confuſedly. This renders it neceſſary to lay aſide ſimple microſcopes, when we wiſh to magnify very conſiderably, and to have recourſe to the compound microſcope.

26th January, 1762.

LETTER LXXXVII.

Bounds, and Defects of the Simple Microſcope.

YOU have now ſeen how ſimple microſcopes may be conſtructed, which ſhall magnify as many times as may be deſired; you have only to meaſure off a ſtraight line of eight inches, like that which I have marked A B,* *(plate VII. fig. 6.)* which contains

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* It being impoſſible here to preſent a ſtraight line of eight inches, one of four is employed for the purpoſe of demonſtration.

I muſt entreat you, then, to recollect what I have already ſuggeſted reſpecting the uſe of the pupil, or that black aperture which we ſee in the eye at the middle of the iris. It is through this aperture that the rays of light are admitted into the eye; accordingly, the larger this aperture is, the more rays are admitted. We muſt here conſider two caſes, in which objects are very luminous and brilliant, and in which they are illuminated by only a very faint light. In the firſt, the pupil contracts of itſelf, without any act of the will, and the Creator has beſtowed on it this faculty, in order to preſerve the interior of the eye from the too dazzling effect of light, which would infallibly injure the nerves. Whenever, therefore, we are expoſed to a very powerful light, we obſerve that the pupil of every eye contracts, to prevent the admiſſion of any more rays into the eye than are neceſſary to paint in it an image ſufficiently luminous. But the contrary takes place when we are in the dark; the pupil, in that caſe, expands to admit the light in a greater quantity. This change is eaſily perceptible every time we paſs from a dark to a luminous ſituation. With reſpect to the ſubject before us, I confine myſelf to this circumſtance, that the more rays of light are admitted into the eye, the more luminous will be the image tranſmitted to the retina, and reciprocally, the ſmaller the quantity of rays which enter the eye, the fainter does the image become, and conſequently the more obſcure does it appear. It may happen, that though the pupil is abundantly expanded, a few rays only ſhall be admitted into the eye. You have only to prick a little hole in a card with a pin, and look at an object through it; and then, however ſtrongly illuminated by the ſun, the object will appear dark in proportion as the aperture is ſmall, nay, it is poſſible to look at the ſun itſelf, employing this precaution. The reaſon is obvious, a few rays only are admitted into the eye: however expanded the pupil may be, the pin-hole in the card determines the quantity of light which enters the eye, and not the pupil, which uſually performs that function.

The ſame thing takes place in the microſcopes which magnify very much; for when the lens is extremely ſmall, a very few rays only are tranſmitted, as *m n* (*plate VII. fig.* 8.) which being ſmaller than the aperture of the pupil, make the object appear ſo much more obſcure; hence, it is evident, that this diminution of light takes place only when the lens MN, or rather it's open part, is ſmaller than the pupil. If it were poſſible to produce a great magnifying effect by means of a greater lens, this obſcurity would not take place; and this is the true ſolution of the queſtion. In order to remedy this inconvenience, in the great effects of the microſcope, care is taken to illuminate the object as ſtrongly as poſſible, to give greater force to the few rays which are conveyed into the eye. To this effect objects are illuminated by the ſun itſelf, mirrors likewiſe are employed, which reflect on them the light of the ſun. Theſe are, nearly, all the circumſtances to be conſidered reſpecting the ſimple microſcope, and by theſe you will eaſily form a judgment of the effect of all thoſe which you may have occaſion to inſpect.

30th January, 1762.

LETTER LXXXVIII.

On Teleſcopes, and their Effect.

BEFORE I proceed to explain the conſtruction of compound microſcopes, a digreſſion reſpecting the teleſcope may perhaps be acceptable. Theſe two inſtruments have a very intimate connection: the one greatly aſſiſts the elucidation of the other. As microſcopes ſerve to aid us in contemplating nearer objects, by repreſenting them under a much greater angle than when viewed at a certain diſtance, ſay eight inches; ſo the teleſcope is employed to aſſiſt our obſervation of very diſtant objects, by repreſenting them under a greater angle than that which they preſent to the naked eye. Inſtruments of this ſort are known by ſeveral names, according to their ſize and uſe; but they muſt be carefully diſtinguiſhed from the glaſſes uſed by aged perſons to relieve the decay of ſight.

A teleſcope magnifies as many times as it repreſents objects under an angle greater than is preſented to the naked eye. The moon, for example, appears to the naked eye under an angle of half a degree, conſequently, a teleſcope magnifies one hundred times, when it repreſents the moon under an angle of fifty degrees, which is one hundred times greater than half a degree. If it magnified two hundred times, it would repreſent the moon under an angle of one hundred degrees: and the moon would, in that caſe, appear to fill more than half of the viſible heavens, whoſe whole extent is only one hundred and eighty degrees.

In common language, we ſay that the teleſcope brings the object nearer to us. This is a very equivocal mode of expreſſion, and admits of two different ſignifications. The one, that on looking through a teleſcope, we conſider the object as many times nearer as it is magnified. But I have already remarked, that it is impoſſible to know the diſtance of objects but by actual meaſurement, and that ſuch meaſurement can be applied only to objects not greatly remote; when, therefore, they are ſo remote as is here ſuppoſed, the eſtimation of diſtance might greatly miſlead us. The other ſignification, which conveys the idea, that teleſcopes repreſent objects as great as they would appear, if we approached nearer to them, is more conformable to truth. You know that the nearer we come to any object, the greater becomes the angle under which it appears; this explanation, accordingly, reverts to that with which I ſet out. When, however, we look at well-known objects, ſay men, at a great diſtance, and view them through a teleſcope under a much greater angle, we are led to imagine ſuch men to be a great deal nearer, as, in that caſe, we would, in effect, ſee them under an angle ſo much greater. But in examining objects leſs approachable, ſuch as the ſun and moon, no meaſurement of diſtance can take place. This caſe is entirely different from that which I have formerly ſubmitted to you, that of a concave lens, employed by near-ſighted perſons, which repreſents the images of objects at a very ſmall diſtance. The concave lens which I uſe, for example, repreſents to me the images of all remote objects, at the diſtance of four inches; it is impoſſible for me, however, to imagine that the ſun, moon, and ſtars are ſo near; accordingly we do not conclude that objects are where their images are found repreſented by glaſſes: we believe this as little as we do the exiſtence of objects in our eyes, though their images are painted there. You will pleaſe to recollect that the eſtimation of the real diſtance, and real magnitude of objects, depends on particular circumſtances.

The principal end of teleſcopes, then, is to increaſe, or multiply, the angle under which objects appear to the naked eye; and the principal diviſion of teleſcopes is eſtimated by the effect which they procure. Accordingly we ſay ſuch a teleſcope magnifies five, another ten, another twenty, another thirty times, and ſo on. And here I remark that pocket-glaſſes rarely magnify beyond ten times; but the uſual teleſcopes employed for examining very diſtant terreſtrial objects magnify from twenty to thirty times; and their length amounts to ſix feet or more. A ſimilar effect, though very conſiderable with regard to terreſtrial objects, is a mere nothing with reſpect to the heavenly bodies, which require an effect inconceivably greater. We have, accordingly, aſtronomical teleſcopes,

ſcopes, which magnify from fifty to two hundred times; and it would be difficult to go farther, as according to the uſual mode of conſtructing them, the greater the effect is, the longer they become. A teleſcope that ſhall magnify one hundred times muſt be at leaſt thirty feet long; and one of a hundred feet in length could ſcarcely magnify two hundred times. You muſt be ſenſible, therefore, that the difficulty of pointing and managing ſuch an unwieldy machine, muſt oppoſe inſurmountable obſtacles to puſhing the experiment farther. The famous Hevelius, the aſtronomer at Dantzick, employed teleſcopes two hundred feet long; but ſuch inſtruments muſt, undoubtedly, have been very defective, as the ſame things are now diſcovered by inſtruments much ſhorter.

This is a brief general deſcription of teleſcopes, and of the different kinds of them, which it is of importance carefully to remark, before we enter into a detail of their conſtruction, and of the manner in which two or more lenſes are united, in order to produce all the different effects.

2d February, 1762.

LETTER LXXXIX.

Of Pocket-Glaſſes.

WE have no certain information reſpecting the perſon to whom we are indebted for the diſcovery of the teleſcope; whether he were a Dutch artiſt, or an Italian of the name of Porta. Whoever will ſee the object diſtinctly. The rays RS, RS, become exactly parallel to each other, becauſe the concave lens has it's focus, or rather it's point of diſperſion, at F.

You have only to recollect that, when parallel rays fall on a concave lens, they become divergent by refraction, ſo that being produced backward, they meet in the focus. This being laid down, we have only to reverſe the caſe, and to conſider the rays SR, SR, as falling on the concave lens; in this caſe it is certain they would aſſume the directions RQ, RQ, which, produced backward, would meet in the point F, which is the common focus of the convex and concave lenſes. Now it is a general law, that in whatever manner rays are refracted in their paſſage from one place to another, they muſt always undergo the ſame refractions in returning from the laſt to the firſt. If, therefore, the refracted rays RQ, RQ, correſpond to the incident rays SR, SR; then, reciprocally, the rays QR, QR, being the incident, the refracted rays will be RS and RS.

The matter will, perhaps, appear in a clearer light ſtill, when I ſay, that concave lenſes have the power of rendering parallel thoſe rays which, without the refraction, would proceed to their focus. You will pleaſe carefully to attend to the following laws of refraction, which apply to both convex and concave lenſes.

I. By a convex lens (*plate VII. fig.* 11.) parallel rays are rendered convergent.

Fig. 1.

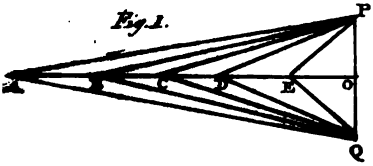


Fig. 2

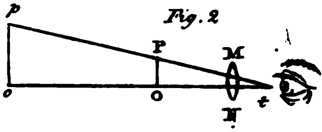


Fig. 3

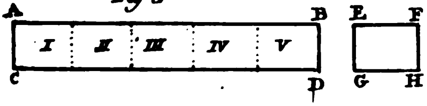


Fig. 4.

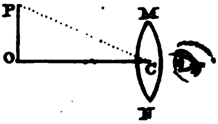


Fig. 6.

Fig 5

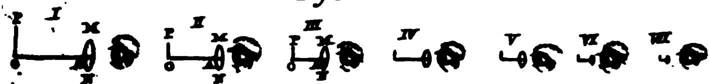


Fig. 8.

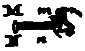


Fig. 7.

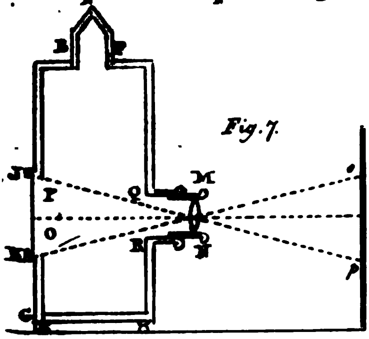


Fig. 9.

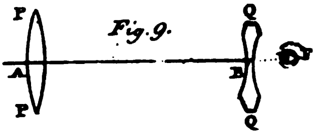


Fig 10.

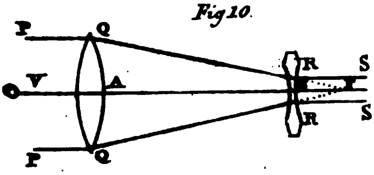
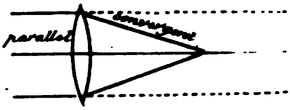


Fig. 11.



Convergent rays become ſtill more ſo, (*plate VIII. fig.* 1.) and divergent leſs divergent.

II. By a concave lens parallel rays are rendered divergent. (*Plate VIII. fig.* 2.)

Divergent rays become ſtill more divergent, *fig.* 3. and convergent rays leſs convergent.

All this is founded on the nature of refraction, and the figure of the lenſes, the diſcuſſion of which would require a very long detail; but the two rules which I have now laid down, contain all that is eſſential. It is abundantly evident, then, that when the convex and the concave lenſes are ſo combined, that they acquire a common focus at F, they will diſtinctly repreſent diſtant objects, becauſe the parallelism of the rays is reſtored by the concave lens, after the convex lens had rendered them convergent. In other words, the rays of very diſtant objects, being nearly parallel to each other, become convergent by a convex lens, and afterwards, the concave lens deſtroys this convergency, and again renders the rays parallel to each other.

6th February, 1762.

LETTER XC.

On the magnifying Power of Pocket Glaſſes

THE principal article reſpecting teleſcopical inſtruments, remains ſtill to be explained, namely, their effect in magnifying objects. I hope to place this in ſo clear a light, as to remove every difficulty in which the ſubject may be involved; and for this purpoſe I ſhall comprize what I have to ſay, in the following propoſitions.

I. Let E *e* (*plate VIII. fig.* 4.) be the object, ſituated on the axis of the inſtrument which paſſes perpendicularly through both lenſes in their centres. This object E *e* muſt be conſidered as at an infinite diſtance.

II. If then the eye, placed at A, looks at this object, it will appear under the angle E A *e*, called it's viſual angle. It will, accordingly, be neceſſary to prove, that on looking at the ſame object through the glaſs, it will appear under a greater angle, and exactly as many times greater as the focal diſtance of the objective lens PAP exceeds that of the ocular QBQ.

III. As the effect of all lenſes conſiſts in repreſenting the objects in another place, and with a certain magnitude, we have only to examine the images which ſhall be ſucceſſively repreſented by the two lenſes, the laſt of which is the immediate object of the ſight of the perſon who looks through the inſtrument.

IV. Now, the object E *e* being infinitely diſtant from the convex lens PAP, it's image will be repreſented behind the lens at F *f*, ſo that AF ſhall be equal to the focal diſtance of the lens; and the magnitude of this image F *f* is determined by the ſtraight line *f* A *e* drawn from the extremity of the object *e* through the centre of the lens A, by which we ſee that this image is inverted, and as many times ſmaller than the object, as the diſtance AF is ſmaller than the diſtance AE.

V. Again, this image F*f* holds the place of the object, relatively to the ocular lens QBQ: as the rays which fall on this lens are preciſely thoſe which would almoſt form the image F*f*, but are intercepted in their progreſs by the concave lens QBQ; ſo that this image is only imaginary; the effect, however, is the ſame as if it were real.

VI. This image F*f*, which we are now conſidering as an object, being at the focal diſtance of the lens QBQ, will be tranſported, almoſt to infinity, by the refraction of this lens. The preceding figure marks this new image at G*g*, whoſe diſtance AG muſt be conceived as infinite, and the rays, refracted a ſecond time by the lens QBQ, will purſue the ſame direction as if they actually proceeded from the image G*g*.

VII. This ſecond image G*g* being, then, the object of the perſon who looks through the inſtrument, it's magnitude falls to be conſidered. To this effect, as it is produced by the firſt image F*f* from the refraction of the lens QBQ, following the general rule, we have only to draw through the centre of the lens B a ſtraight line, which ſhall paſs through the point *f* of the firſt image, and that line will mark, at *g*, the extremity of the ſecond image.

VIII. Let the ſpectator now apply his eye to B; and as the rays which it receives purſue the ſame direction as if they actually proceeded from the im[illegible] G*g*, it will appear to him un[illegible] angle G[illegible] which is greater than the angle EA*e*, under which the object E*e* appears to the naked eye.

IX. In order the better to compare theſe two angles, it is evident, firſt, that the angle EA*e* is equal to the angle FA*f*, being vertical angles; for the ſame reaſon the angle GB*g* is equal to the angle FB*f*, being vertical and oppoſite at the point B. It remains to be proved, therefore, that the angle FB*f* exceeds the angle FA*f* as many times as the line AF exceeds the line B*f*; the former of which, AF, is the focal diſtance of the objective, and the other, BF, the focal diſtance of the ocular.

X. In order to demonſtrate this, we muſt have recourſe to certain geometrical propoſitions reſpecting the nature of ſectors. You will recollect that the ſector is part of a circle contained between two radii CM and CN, and an arch or portion of the circumference MN. In a ſector, then, there are three things to be conſidered; 1. The radius of the circle, CM or CN: 2. The quantity of the arch MN: 3. The angle MCN.

XI. Let us now conſider two ſectors, MCN and *mcn (plate VIII. fig. 5.)* whoſe radii CM and *cm* are equal to each other: now it is demonſtrated in the elements of geometry, that the angles C and *c* have the ſame proportion to each other, that the arches MN and *mn* have; in other words, the angle C is as many times greater than the angle *c*, as the arch MN is greater than the arch *mn*: but, inſtead of this aukward mode of expreſſion, we ſay, that the angles C and *c* are proportional to the arches MN and *m n*, the radii being equal.

XII. Let us likewiſe conſider two ſectors, MCN and *m c n* (*fig.* 6.) whoſe angles C and *c* are equal to each other, but the radii unequal: and it is demonſtrated in geometry, that the arch MN is as many times greater than the arch *m n*, as the radius CM is greater than the radius *c m*: or, in geometrical language, the arches are in proportion to the radii, the angles being equal. The reaſon is obvious; for every arch contains as many degrees as it's angle; and the degrees of a great circle exceed thoſe of a ſmall one as many times as the greater radius exceeds the ſmaller.

XIII. Finally, let us conſider likewiſe the caſe when, as in the two ſectors MCN and *m c n* (*fig.* 7.) the arches MN and *m n* are equal, but the radii CM and *c m* unequal.

In this caſe, the angle C, which correſponds to the greater radius CM, is the ſmaller, and the angle *c*, which correſponds to the ſmaller radius *c m*, is the greater, and this in the ſame proportion as the radii. That is, the angle *c* is as many times greater than the angle C, as the radius CM is greater than the radius *c m*: or, to ſpeak geometrically, the angles are reciprocally proportional to the radii, the arches being equal.

XIV. This laſt propoſition carries me forward to my concluſion, after I have ſubjoined this remark, that when the angles are very ſmall, as in the caſe of pocket-glaſſes, there is no ſenſible difference in the chords of the arches MN and mn, that is of the ſtraight lines MN, and mn.

XV. Having made this remark, we return to *fig.* 4. The triangles FAf and FBf may be conſidered as ſectors, in which the arch Ff is the ſame in both. Conſequently the angle FBf exceeds the angle FAf as often as the diſtance AF exceeds the diſtance BF. That is, the object Ee will appear through the inſtrument, under an angle as many times greater as the focal diſtance of the objective AF exceeds the focal diſtance of the ocular BF; which was the thing to be demonſtrated.

9th February, 1762.

LETTER XCI.

Defects of Pocket Glaſſes. Of the apparent Field.

YOU muſt be ſenſible that no great advantage is to be expected from ſuch ſmall inſtruments; and it has already been remarked that they do not magnify objects above ten times. Were the effect to be carried farther, not only would the length become too great to admit of their being carried about in the pocket, but they would become ſubject to other and more eſſential defects. This has induced artiſts entirely to lay aſide glaſſes of this ſort, when ſuperior effect is required.

The principal of theſe defects is the ſmallneſs of the apparent field; and this leads me forward to explain an important article relating to teleſcopes of every deſcription. When a teleſcope is directed toward the heavens, or to very diſtant objects on the earth, the ſpace diſcovered appears in the figure of a circle, and we ſee thoſe objects only which are included in that ſpace; ſo that if you wiſhed to examine other objects, the poſition of the inſtrument muſt be altered. This circular ſpace, preſented to the eye of the ſpectator, is denominated the *apparent field*, or, in one word, *the field* of the inſtrument: and it is abundantly obvious, that it muſt be a great advantage to have a very large field, and that, on the contrary, a ſmall field is a very great inconvenience, in inſtruments of this ſort. Let us ſuppoſe two teleſcopes directed toward the moon, by the one of which we can diſcover only the half of that luminary, whereas by the other we ſee her whole body, together with the neighbouring ſtars; the field of this laſt is, therefore, much greater than that of the other. That which preſents the greater field relieves us not only from the trouble of frequently changing the poſition, but procures another very great advantage; that of enabling us to compare, by viewing them at the ſame time, ſeveral parts of the object, one with another.

It is, therefore, one of the greateſt perfections of a teleſcope to preſent a very ample field; and it is, accordingly, a matter of much importance to meaſure the field of every inſtrument. In this view, we are regulated by the heavens, and we determine the circular ſpace ſeen through a teleſcope, by meaſuring it's diameter in degrees and minutes. Thus, the apparent diameter of the full moon, being about half a degree, if a teleſcope takes in the moon only, we ſay that the diameter of it's field is half a degree; and if you could ſee at once only the half of the moon, the diameter of the field would be the quarter of a degree.

The meaſurement of angles, then, furniſhes the means of meaſuring the apparent field; beſides, the thing is ſufficiently clear of itſelf. Suppoſing we could ſee through the inſtrument AB (*plate* VIII, *fig.* 8.) only the ſpace POP, and the objects which it contains: this ſpace being a circle, it's diameter will be the line POP, whoſe middle point O is in the axis of the inſtrument. Drawing, therefore, from the extremities PP the ſtraight lines PC, PC, the angle PCP will expreſs the diameter of the apparent field, and the half of this angle OCP is denominated the ſemi-diameter of the apparent field of ſuch an inſtrument. You will perfectly comprehend the meaning, then, when it is ſaid that the diameter of the apparent field of ſuch an inſtrument is one dedegree, that of another two degrees, and ſo on; as alſo when it is marked by minutes, as 30 minutes which make half a degree, or 15 minutes which make the fourth part of a degree.

But in order to form a right judgment of the value of a teleſcope, with reſpect to the apparent field, we muſt likewiſe attend to the magnifying power of the inſtrument. It may be remarked, in general, that the more a teleſcope magnifies, the ſmaller, of neceſſity, muſt be the apparent field; theſe are the bounds which nature herſelf has preſcribed. Let us ſuppoſe an inſtrument which ſhould magnify 100 times; it is evident that the diameter of the field could not poſſibly be ſo much as two degrees: for, as this ſpace would appear 100 times greater, it would reſemble a ſpace of two hundred degrees; greater, of conſequence, than the whole viſible heavens, which, from the one extremity to the other, contain only 180 degrees, and of which we can ſee but the half at moſt at once, that is a circular ſpace of 90 degrees in diameter. From this you ſee, that a teleſcope which magnifies 100 times could not contain a field of ſo much as one degree; for this degree multiplied 100 times would give more than 90 degrees; and that, accordingly, a teleſcope which magnified 100 times would be excellent, if the diameter of it's field were ſomewhat leſs than one degree: and the very nature of the inſtrument admits not of a greater effect.

But another teleſcope, which ſhould magnify only 10 times, would be extremely defective, if it diſcovered a field of only one degree in diameter; as this field magnified 10 times would give a ſpace of no more than ten degrees in the heavens, which would be a ſmall matter, by ſetting too narrow bounds to our view. We ſhould have good reaſon, then, to reject ſuch an inſtrument altogether. Thus it would be very eaſy, with reſpect to the apparent field, to form a judgment of the excellence or defectiveneſs of inſtruments of this ſort, when the effect is taken into conſideration. For when it magnifies only 10 times, it may fairly be conjectured, that it diſcovers a field of 9 degrees; as 9 degrees taken 10 times give 90 degrees, a ſpace which our ſight is capable of embracing: and if the diameter of it's field were only 5 degrees, or leſs, this would be an inſtrument very defective indeed. Now I ſhall be able to demonſtrate, that if a teleſcope were to be conſtructed ſuch as I have been deſcribing, which ſhould magnify more than 10 times, it would be liable to this defect: the apparent field multiplied by the magnifying power would be very conſiderably under 90 degrees, and would not even ſhew the half. But when a ſmall effect is aimed at, this defect is not ſo ſenſible; for if ſuch an inſtrument magnifies only 5 times, the diameter of it's field is about 4 degrees, which, magnified 5 times, contains a ſpace of 20 degrees, with which we have reaſon to be ſatisfied: but if we wiſhed to magnify 25 times, the diameter of the field would be only half a degree, which taken 25 times, would give little more than 12 degrees, which is too little. When therefore we would magnify very much, a different arrangement of lenſes muſt be employed, which I ſhall afterward explain.

13th February, 1762.

LETTER XCII.

Determination of the apparent Field for Pocket Glaſſes.

TO aſcertain the apparent field being of very great importance in the conſtruction of teleſcopes, I proceed to the application of it to the ſmall glaſſes which I have been deſcribing.

The lens P A P, *(plate VIII. fig.* 4.) is the objective, Q B Q the ocular, and the ſtraight line E F the axis of the inſtrument, in which is ſeen, at a very great diſtance, through the inſtrument, the object E *e*, under the angle E A *e*, which repreſents the ſemidiameter of the apparent field, for it extends as far on the other ſide downward. The point E, then, is the centre of the ſpace ſeen through the inſtrument, the radius of which, E A, as it paſſes perpendicularly through both lenſes, undergoes no refraction; and in order that this ray may have admiſſion into the eye, the eye muſt be fixed ſomewhere on the axis of the inſtrument B F, behind the ocular lens, ſo that the centre of the pupil ſhall be in the line B F; and this is a general rule for every ſpecies of teleſcope. Let us now conſider the viſible extremity of the object *e*, whoſe rays exactly fill the whole opening of the objective lens P A P; but it will be ſufficient to attend only to the ray E A, which paſſes through the centre of the objective A, as the others ſurround, and little more than ſtrengthen this ray; ſo that if it is admitted into the eye, the others, or at leaſt a conſiderable part of them, find admiſſion likewiſe; and if this ray is not admitted into the eye, though perhaps ſome of the others may enter, they are too feeble to excite an impreſſion ſufficiently powerful. Hence, this may be laid down as a rule, that the extremity *e* of the object is ſeen, only ſo far as the ray *e* A, after having paſſed through the two lenſes, is admitted into the eye.

We muſt, therefore, carefully examine the direction of this ray *e* A. Now, as it paſſes through the centre of the objective A, it undergoes no refraction; conformably to the rule laid down from the beginning, That rays paſſing through the centre of any lens whatever are not diverted from their direction, that is, undergo no refraction. This ray, *e* A, therefore, after having paſſed through the objective, would continue in the ſame direction, to meet the other rays iſſuing from the ſame point *e*, to the point *f* of the image repreſented by the objective at F *f*, the point *f* being the image of the point *e* of the object; but the ray meeting, at *m*, the concave lens, but not in it's centre, will be diverted from that direction; and inſtead of terminating in *f*, will aſſume the direction *m n*, more divergent from B F, it being the natural effect of concave lenſes to render rays always more divergent. In order to aſcertain this new direction *m n*, you will pleaſe to recollect that the objective lens repreſents the object E *e* in an inverted poſition at F *f*, ſo that A F is equal to the focal diſtance of this lens, which tranſports the object E *e* to F *f*. Then this image F *f* occupies the place of the object, with reſpect to the ocular lens Q B Q, which, in it's turn, tranſports that image to G *g*, whoſe diſtance B G muſt be as great as that of the object itſelf; and for this effect, it is neceſſary to place the ocular lens in ſuch a manner that the interval B F ſhall be equal to it's focal diſtance.

As to the magnitude of theſe images, the firſt F *f* is determined by the ſtraight line *e* A *f* drawn from *e* through the centre A of the firſt lens; and the other G *g* by the ſtraight line *f* B *g* drawn from the point *f* through the centre B of the ſecond lens. This being laid down, the ray A *m* directed toward the point *f* is refracted, and proceeds in the direction *m n*; and this line *m n* being produced backward will paſs through the point *g*, for *m n* has the ſame effect in the eye, as if it actually proceeded from the point *g*. Now, as this line *m n* retires farther and farther from the axis B F, where the centre of the pupil is, it cannot enter into the eye, unleſs the opening of the pupil extends ſo far; and if the opening of the pupil were reduced to nothing, the ray *m n* would be excluded from the eye, and the point *e* of the object could not be viſible, nor even any other point of the object out of the axis A F. There would, therefore, be no apparent field, and nothing would be ſeen, through ſuch an inſtrument, except the ſingle point E of the object, which is in it's axis. It is evident, then, that a teleſcope of this ſort diſcovers no field, but as far as the pupil expands, ſo that in proportion as the expanſion of the pupil is greater or leſs, ſo likewiſe the apparent field is great or ſmall.

In

In this caſe, the point *e* will therefore be ſtill viſible to the eye, if the ſmall interval B *m* does not exceed half the diameter of the eye, that the ray *m n* may find admiſſion into it; but in this caſe, likewiſe, the eye muſt be brought as cloſe as poſſible to the ocular lens: for as the ray *m n* removes from the axis F B, it would eſcape the pupil at a greater diſtance.

Now it is eaſy to determine the apparent field which ſuch an inſtrument would diſcover on the ocular lens: you have only to take the interval B *m* equal to the ſemi-diameter of the pupil, and to draw through that point *m*, and the centre of the objective lens A, the ſtraight line *m* A *e*, then this line will mark on the object the extremity *e*, which will be ſtill viſible through the inſtrument, and the angle E A *e* will give the ſemi-diameter of the apparent field. Hence you will eaſily judge, that whenever the diſtance of the lenſes A B exceeds ſome inches, the angle B A *m* muſt become extremely ſmall, as the line or the diſtance B *m* is but about the twentieth part of an inch. Now if it were intended to magnify very much, the diſtance of the lenſes muſt become conſiderable, and the conſequence would be, that the apparent field muſt become extremely ſmall. The ſtructure of the human eye, then, ſets bounds to teleſcopes of this deſcription, and obliges us to have recourſe to others of a different conſtruction, whenever we want to produce very conſiderable effect.

16th February, 1762.

LETTER

LETTER XCIII.

Aſtronomical Teleſcopes, and their magnifying Powers.

I PROCEED to the ſecond ſpecies of teleſcopes, called aſtronomical, and remark that they conſiſt of only two lenſes, like thoſe of the firſt ſpecies; with this difference, that in the conſtruction of aſtronomical teleſcopes, inſtead of a concave ocular lens, we employ a convex.

The objective P A P (*plate VIII. fig.* 9.) is, as in the other ſpecies, convex, whoſe focus being at F, we fit, on the ſame axis, a ſmaller convex lens Q Q, in ſuch a manner that it's focus ſhall likewiſe fall on the ſame point F. Then placing the eye at O, ſo that the diſtance B O ſhall be nearly equal to the focal diſtance of the ocular Q Q, you will ſee objects diſtinctly, and magnified as many times as the focal diſtance of the objective A F ſhall exceed that of the ocular B F: but it is to be remarked that every object will appear in an inverted poſition, ſo that if the inſtrument were to be pointed toward a houſe, the roof would appear undermoſt, and the ground-floor uppermoſt. As this circumſtance would be aukward in viewing terreſtrial objects, which we never ſee in an inverted ſituation, the uſe of this ſpecies of teleſcopes is confined to the heavenly bodies, it being a matter of indifference in what direction they appear: it is ſufficient to the aſtronomer to know that what he ſees uppermoſt is really undermoſt, and recipro-

cally.

cally. Nothing, however, forbids the application of ſuch teleſcopes to terreſtrial objects; the eye ſoon becomes accuſtomed to the inverted poſition, provided the object is ſeen diſtinctly and very much magnified.

Having given this deſcription, three things fall to be demonſtrated: firſt, that by this arrangement of the lenſes objects muſt appear diſtinctly; ſecondly, that they muſt appear magnified as many times as the focal diſtance of the objective lens exceeds that of the ocular, and in an inverted poſition; and thirdly, that the eye muſt not be applied cloſe to the ocular lens, as in the firſt ſpecies, but muſt be removed to nearly the focal diſtance of the ocular.

1. As to the firſt, it is demonſtrated in the ſame manner as in the preceding caſe: the rays *e* P, *e* P, which are parallel before they enter into the objective lens, meet by refraction in the focus of this lens at F; the ocular lens muſt, of courſe, reſtore the paralleliſm of theſe rays, and diſtinct viſion requires that the rays, proceeding from every point, ſhould be nearly parallel to each other when they enter the eye. Now, the ocular lens, having it's focus at F, is placed in ſuch a manner as to render the rays F M, F M, by the refraction, parallel, and conſequently the eye will receive the rays N *o*, N *o*, parallel to each other.

2. With reſpect to the ſecond article, let us conſider the object at E *e*, (*plate VIII. fig.* 10.) but ſo as that the diſtance E A ſhall be almoſt infinite. The image of this object, repreſented by the objective

lens, will therefore be F *f*, ſituated at the focal diſtance of that lens A F, and determined by the ſtraight line *e* A *f*, drawn through the centre of the lens. This image F *f*, which is inverted, occupies the place of the object with reſpect to the ocular lens; and being in it's focus, the ſecond image will be again removed to an infinite diſtance by the refraction of this lens, and will fall, for example, at G *g*, the diſtance A G being conſidered as infinite, like that of A E. Now, in order to determine the magnitude of this image, you have only to draw through the centre B of the lens, and the extremity *f* of the firſt image, the ſtraight line B *f g*. Now this ſecond image G *g* being the immediate object of viſion to the perſon who looks through the teleſcope, it is evident at once that this repreſentation is inverted; and, as it is infinitely diſtant, will appear under an angle G B *g*. But the object itſelf E *e* will appear to the naked eye under the angle E A *e*: now you are ſenſible, without being reminded, that it is indifferent to take the points A and B, in order to have the viſual angles E A *e* and G B *g*, on account of the infinite diſtance of the object. You now ſee here, as in the preceding caſe, that the triangles F A *f* and F B *f* may be conſidered as circular ſectors, the line F *f* meaſuring the arch of both, and the angles themſelves being ſo very ſmall, no ſenſible miſtake can be committed in taking the chord for the arch. As, then, the radii of theſe two ſectors are the lines A E and B F, the arches being equal to each other, it follows, as was formerly demonſtrated, that the angles F A *f* (or, which is the ſame thing, E A *e*) and F B *f* (or, which is the ſame thing, G B *g*) have the ſame proportion to each other that the radii B F and A F have. Therefore, the angle G B *g*, under which the object is ſeen through the teleſcope, as many times exceeds the angle E A *e*, under which the object is ſeen by the naked eye, as the line A F exceeds the line B F; which was the ſecond point to be demonſtrated. I am under the neceſſity of deferring the demonſtration of my third propoſition till next poſt.

20th February, 1762.

LETTER XCIV.

Of the apparent Field, and the Place of the Eye.

IN fulfilling my engagement reſpecting the third particular propoſed, namely to determine the place of the eye behind the teleſcope, I remark that this article is moſt intimately connected with the apparent field, and that it is preciſely the field which obliges us to keep the eye fixed at the proper diſtance; for if it were to be brought cloſer, or removed farther off, we ſhould no longer diſcover ſo large a field.

The extent of the field being an article of ſuch importance, indeed ſo eſſential, in all teleſcopes, it muſt be of equal importance to determine exactly the place of the eye from which the largeſt field is diſcoverable. If the eye were to be applied cloſe to the ocular lens, we ſhould have nearly the ſame field as we have

with

Fig 1.

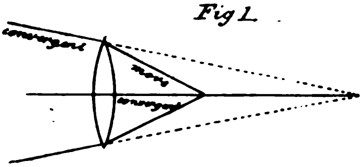


Fig 2

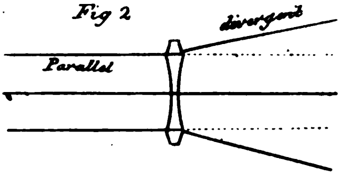


Fig. 3.

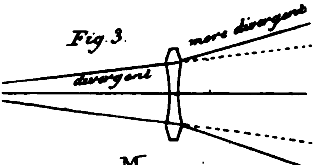


Fig 4

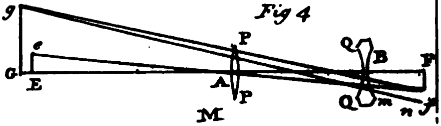


Fig 5

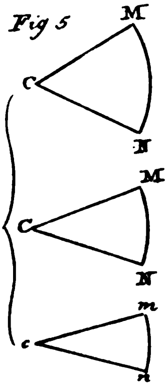


Fig. 6.

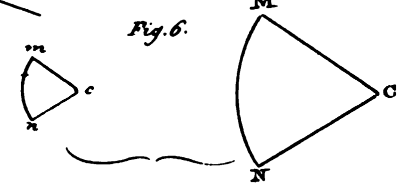


Fig 7

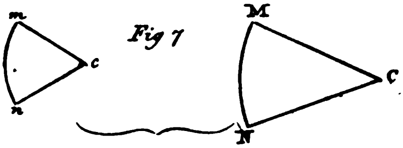


Fig 8

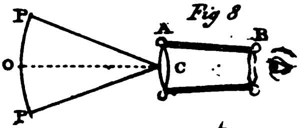


Fig. 9

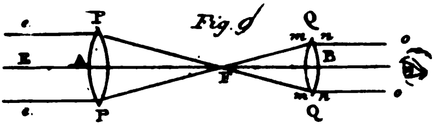


Fig 10

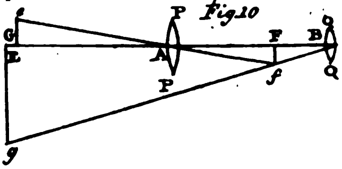
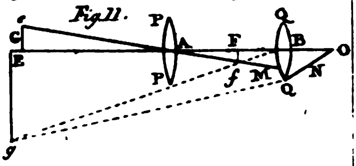


Fig 11.



with the pocket-glaſs, which becomes inſufferably ſmall, whenever the magnifying power is conſiderable. It is, therefore, a vaſt advantage to aſtronomical teleſcopes, that by withdrawing the eye from the ocular lens, the apparent field increaſes to a certain extent: and it is preciſely this which renders ſuch teleſcopes ſuſceptible of prodigious magnifying powers, whereas thoſe of the firſt ſpecies are, in this reſpect, extremely limited. You know that with the aſtronomical teleſcope, the magnifying power has been carried beyond two hundred times, which gives them an inconceivable ſuperiority over thoſe of the firſt ſpecies, which can ſcarcely magnify ten times; and the trifling inconvenience of the inverted poſition is infinitely overbalanced by an advantage ſo very great.

I will endeavour to put this important article in the cleareſt light poſſible.

1. The object E *e* (*plate VIII. fig.* 11.) being infinitely diſtant, let *e* be it's extremity, ſtill viſible through the teleſcope, whoſe lenſes are P A P and Q B Q, fitted on the common axis E A B O, it falls to be attentively conſidered what direction will be purſued by the ſingle ray which paſſes from the extremity *e* of the object, through the centre A of the objective lens. You will recollect that the other rays, which fall from the point *e* on the objective lens only accompany and ſtrengthen the ray in queſtion *e* A, which is the principal with reſpect to viſion.

2. Now this ray *e* A, paſſing through the centre

of the lens P P, will undergo no refraction, but will purſue it's direction in the ſtraight line A *f m*, and paſſing through the extremity of the image F *f* will fall on the ocular lens at the point *m*; and here it is to be obſerved, that if the ſize of the ocular lens had not extended ſo far as the point *m*, this ray would never have reached the eye, and the point *e* would have been inviſible. That is to ſay, it would be neceſſary to take the extremity *e* nearer to the axis, in order that the ray A *f m* may meet the ocular lens.

3. Now this ray A *m* will be refracted, by the ocular lens, in a way which it is very eaſy to diſcover. We have only to conſider the ſecond image G *g*, though infinitely diſtant, it is ſufficient to know that the ſtraight line B *f* produced will paſs through the extremity *g* of the ſecond image G *g*, which is the immediate object of viſion. Having remarked this, the refracted ray muſt aſſume the direction *n* O, and this produced paſſes through *g*.

4. As, therefore, the two lines O *n* and B *f* meet at an infinite diſtance at *g*, they may be conſidered as parallel to each other; and hence we acquire an eaſier method to determine the poſition of the refracted ray *n* O: you have only to draw it parallel to the line B *f*.

5. Hence it is clearly evident that the ray *n* O will ſomewhere meet the axis of the teleſcope at O, and as uſually, when the magnifying power is great, the point F is much nearer to the lens Q Q than to the lens P P, the diſtance B *m* will be ſomewhat greater

than the image F *f*: and as the line *n* O is parallel to *f* B, the line B O will be nearly equal to B F, that is, to the focal diſtance of the ocular lens.

6. If, then, the eye is placed at O, it will receive not only the rays which proceed from the middle of the object E, but thoſe likewiſe which proceed from the extremity *e*, and conſequently, thoſe alſo which proceed from every point of the object; the eye would even receive at once the rays B O and *n* O, even ſuppoſing the pupil infinitely contracted. In this caſe, therefore, the apparent field does not depend on the largeneſs of the aperture of the pupil, provided the eye be placed at O, but the moment it recedes from this point, it muſt loſe conſiderably in the apparent field.

7. If the point *m* were not in the extremity of the ocular lens, it would tranſmit rays ſtill more remote from the axis, and the teleſcope would, of courſe, diſcover a larger field. In order then, to determine the real apparent field which the teleſcope is capable of diſcovering, let there be drawn, from the centre A of the objective lens, to the extremity *m* of the ocular, the ſtraight line A *m*, which, produced to the object, will mark at *e* the viſible extremity; and conſequently the angle E A *e*, or, which is the ſame thing, the angle B A *m*, will give the ſemi-diameter of the apparent field, which is, conſequently, greater in proportion as the extent of the ocular lens is greater.

8. As, then, in the firſt ſpecies of teleſcopes, the apparent field depended entirely on the aperture of

the quotient is always equal to the dividend; if then it is aſked, how often one inch is contained in twenty-four inches, the anſwer, without heſitation, is, twenty-four times; conſequently, ſuch a teleſcope magnifies twenty-four times, that is, repreſents diſtant objects in the ſame manner as if they were twenty-four times greater than they really are; in other words, you would ſee them through ſuch teleſcope under an angle twenty-four times greater than by the naked eye.

Let us ſuppoſe another aſtronomical teleſcope, the focal diſtance of whoſe objective lens is thirty-two feet, and that of the ocular three inches. You ſee at once that theſe two lenſes muſt be placed at the diſtance of thirty-two feet, and three inches from each other, for, in all aſtronomical teleſcopes, the diſtance of the lenſes muſt be equal to the ſum of the two focal diſtances, as has been already demonſtrated.

To find, then, how many times a teleſcope of the above deſcription magnifies, we muſt divide thirty-two feet by three inches, and, in order to this, reduce theſe thirty-two feet into inches, by multiplying them by twelve.

32 this produces 384 inches; and theſe again
12 divided by three, the focal diſtance, in inches,
3|384 of the ocular lens, gives a quotient of 128;
128 which indicates that the propoſed teleſcope
magnifies 128 times, which muſt be allowed to be very conſiderable.

Reciprocally, therefore, in order to conſtruct a teleſcope which ſhall magnify a given number of times, ſay 100, we muſt employ two convex lenſes, the focal diſtance of the one of which ſhall be 100 times greater than that of the other; in this caſe the one will give the objective lens, and the other the ocular. Theſe muſt afterwards be fitted on the ſame axis, ſo that their diſtance ſhall be equal to the ſum of the two focal diſtances; that is, they muſt be fixed in a tube of this length, and then the eye being placed behind the ocular lens, at it's focal diſtance, will ſee objects magnified 100 times.

This arrangement may be varied without end, by aſſuming an ocular lens at pleaſure, and adapting to it an objective, whoſe focal diſtance ſhall be 100 times greater. Thus, taking an ocular lens of one inch focus, the objective muſt be of 100 inches focus, and the diſtance of the lenſes 101 inches. Or, taking an ocular of 2 inches focus, the objective muſt have it's focus at the diſtance of 200 inches, and the diſtance of the lenſes will be 202 inches. If you were to take an ocular lens of 3 inches focus, the focal diſtance of the objective muſt be 300 inches, and the diſtance of the lenſes from each other 303 inches. And if you were to take an ocular lens of 4 inches focus, the objective muſt have a focal diſtance of 400 inches, and the diſtance of the two lenſes 404 inches, and ſo on, the inſtrument always increaſing in length. If, on the contrary, you were to aſſume an ocular lens of only half an inch focus, the objective muſt have a focal diſtance of 100 half inches, that is, of fifty inches, and the diſtance between the lenſes would only be 50 [illegible] which [illegible] more than four feet.

And if an ocular of a quarter of an inch focus were to be employed, the objective would require a focal diſtance of only 100 quarters of an inch, or 25 inches, and the diſtance between the two lenſes 25 inches and a quarter, that is little more than two feet.

Here, then, are ſeveral methods of producing the ſame effect, that of magnifying 100 times; and if every thing elſe were equal, we ſhould not heſitate about giving the preference to the laſt, as being the ſhorteſt, for here the teleſcope, being reduced to little more than two feet, would be more manageable than one much longer.

No one, then, would heſitate about preferring the ſhorteſt teleſcopes, provided all other circumſtances were the ſame, and all the different ſpecies repreſented objects in the ſame degree of perfection. But, though they all poſſeſs the ſame magnifying power, the repreſentation is by no means equally clear and diſtinct. That of two feet in length certainly magnifies 100 times, as well as the others; but on looking through ſuch a teleſcope, objects will appear not only dark, but blunt and confuſed, which is undoubtedly a very great defect. The laſt teleſcope but one, whoſe objective lens is 50 inches focus, is leſs ſubject to theſe defects, but the dimneſs and confuſion are ſtill inſupportable: and theſe defects diminiſh in proportion as we employ greater objective lenſes; and are reduced to almoſt nothing, on employing an objective lens of 300 inches, with an ocular of 3 inches focus. On increaſing theſe meaſurements, the repreſentation becomes ſtill clearer and more diſtinct; ſo that, in this reſpect, long teleſcopes are preferable to ſhort, though otherwiſe leſs commodious. This circumſtance impoſes on me a new taſk, that of farther explaining two very eſſential articles in the theory of teleſcopes: the one reſpects the clearneſs, or degree of light in which objects are ſeen: and the other the diſtinctneſs and accuracy of expreſſion with which they are repreſented. Without theſe two qualities, all magnifying power, however great, procures no advantage for the contemplation of objects.

27th February, 1762.

LETTER XCVI.

Degree of Clearneſs.

IN order to form a judgment of the degree of clearneſs in which objects are repreſented by the teleſcope, I ſhall recur to the ſame principles which I endeavoured to elucidate, in treating the ſame ſubject with reference to the microſcope.

And, firſt, it muſt be conſidered that, in this reſearch, it is not propoſed to determine the degree of light reſident in objects themſelves, and which may be very different, not only in different bodies, as being in their nature more or leſs luminous, but in the ſame body, according as circumſtances vary. The ſame bodies, when illuminated by the ſun, have undoubtedly more light than when the ſky is overcaſt, and in the night their light is wholly extinguiſhed;

but different bodies illuminated may differ greatly, in point of brightneſs, according as their colours are more or leſs lively. We are not enquiring, then, into that light or brightneſs which reſides in objects themſelves; but, be it ſtrong or faint, we ſay that a teleſcope repreſents the object in perfect clearneſs, when it is ſeen through the inſtrument as clearly as by the naked eye; ſo that if the object be dim, we are not to expect that the teleſcope ſhould repreſent it as clear.

Accordingly, in reſpect of clearneſs, a teleſcope is perfect, when it repreſents the object as clearly as it appears to the naked eye. This takes place, as in the microſcope, when the whole opening of the pupil is filled with the rays which proceed from every point of the object, after being tranſmitted through the teleſcope. If a teleſcope furniſhes rays ſufficient to fill the whole opening of the pupil, no greater degree of clearneſs need be deſired; and, ſuppoſing it could ſupply rays in greater profuſion, this would be entirely uſeleſs, as the ſame quantity preciſely, and no more, could find admiſſion into the eye.

Here, then, attention muſt be paid chiefly to the aperture of the pupil, which, being variable, prevents our laying down a fixed rule, unleſs we regulate ourſelves according to a certain given aperture, which is ſufficient, when the pupil, in a ſtate of the greateſt contraction, is filled with rays; and, for this purpoſe, the diameter of the pupil is uſually ſuppoſed to be one line, twelve of which make an inch; we ſometimes

times ſatisfy ourſelves with even the half of this, allowing to the diameter of the pupil only half a line, and in ſome caſes ſtill leſs.

If you will pleaſe to conſider, that the light of the ſun exceeds that of the moon 300,000 times, though even that of the moon is by no means inconſiderable, you will be ſenſible that a ſmall diminution in point of clearneſs can be of no great conſequence in the contemplation of objects. Having premiſed this, all that remains is to examine the rays which the teleſcope tranſmits into the eye, and to compare them with the pupil; and it will be ſufficient to conſider the rays which proceed from a ſingle point of the object *(plate IX. fig. 1.)* that, for example, which is in the axis of the teleſcope.

I. The object being infinitely diſtant, the rays which fall from it on the ſurface of the objective lens P A P are parallel to each other: all the rays, then, which come from the centre of the object, will be contained within the lines L P, L P, parallel to the axis E A. All theſe rays taken together are denominated the *faſcicle* of rays which fall on the objective lens, and the breadth of this faſcicle is equal to the extent or aperture of the objective lens, the diameter of which is P A P.

II. This faſcicle, or little bundle, of rays is changed, by the refraction of the objective lens, into a conical or pointed figure P F P, and having croſſed at the focus F, it forms a new cone *m* F *m*, terminated by the ocular lens; hence it is evident that the baſis of this cone *m m* is as many times ſmaller than the breadth of the faſcicle P P, as the diſtance F B is ſhorter than the diſtance A F.

III. Now theſe rays F m, F m, on paſſing through the ocular lens Q B Q, become again parallel to each other, and form the faſcicle of rays $n o$, $n o$, which enter into the eye, and there depict the image of the point of the object whence they originally proceeded.

IV. The queſtion, then, reſolves itſelf into the breadth of this faſcicle of rays $n o$, $n o$, which enter into the eye; for if this breadth $n n$ or $o o$ is equal to, or greater than, the opening of the pupil, it will be filled with them, and the eye will enjoy all poſſible clearneſs; that is, the object will ſeem as clear as if you were to look at it with the unaſſiſted eye.

V. But if this faſcicle $n n$, or $o o$, were of much leſs breadth than the diameter of the pupil, it is evident that the repreſentation muſt become ſo much more obſcure; which would be a great defect in the teleſcope. In order to remedy it, the faſcicle muſt, therefore, be at leaſt half a line in breadth, and it would be ſtill better to have it a whole line in breadth, this being the uſual aperture of the pupil.

VI. It is evident that the breadth of this ſecond faſcicle has a certain relation to that of the firſt, which it is very eaſy to determine. You have only to ſettle how many times the diſtance $n n$ or $m m$ is leſs than the diſtance P P, which is the aperture of the objective lens. But, the diſtance P P is in the ſame proportion to the diſtance $m m$, as the diſtance A F to the diſtance B F, on which the magnifying power depends; accordingly, the magnifying power itſelf diſcovers how many times the faſcicle LP, LP, is broader than the faſcicle *n o*, *n o*, which enters into the eye.

VII. Since, then, the breadth *n n* or *o o* muſt be one line, at leaſt half a line, the aperture of the objective lens PP muſt at leaſt contain as many half lines as the magnifying power indicates; thus, when the teleſcope is to magnify 100 times, the aperture of it's objective lens muſt have a diameter of 100 half lines, or 50 lines, which make 4 inches and 2 lines.

VIII. You ſee, then, that, in order to avoid obſcurity, the aperture of the objective lens muſt be greater, in proportion as the magnifying power is greater. And, conſequently, if the objective lens employed is not ſuſceptible of ſuch an aperture, the teleſcope will be defective in reſpect of clearneſs of repreſentation.

Hence it is abundantly evident, that, in order to magnify very greatly, it is impoſſible to employ ſmall objectives, whoſe focal diſtance is too ſhort, as a lens formed by the arches of ſmall circles cannot have a great aperture.

1ſt March, 1762.

LETTER XCVII.

Aperture of Objective Lenſes.

YOU have now ſeen that the magnifying power determines the ſize or extent of the objective lens, in order that objects may appear with a ſufficient degree of clearneſs. This determination reſpects only the ſize or aperture of the objective lens; however the focal diſtance is affected by it likewiſe, for the larger the lens is, the greater muſt be it's focal diſtance.

The reaſon of this is evident, as in order to form a lens whoſe focal diſtance is, for example, two inches, it's two ſurfaces muſt be arches of a circle whoſe radius is likewiſe about two inches: I have therefore repreſented (*plate IX. fig.* 2.) two lenſes P and Q, the arches of which are deſcribed with a radius of two inches. The lens P, being the thicker, is much greater than the lens Q; but I ſhall demonſtrate afterwards that thick lenſes are ſubject to other inconveniencies, and theſe ſo great as to oblige us to lay them altogether aſide. The lens Q, then, will be found more adapted for uſe, being compoſed of ſmaller arches of the ſame circle; and as it's focal diſtance is two inches, it's extent or aperture *m n* may ſcarcely exceed one inch. Hence this may be laid down as a general rule, that the focal diſtance of a lens muſt always be twice greater than the diameter of it's aperture *m n*; that is, the aperture of a lens muſt, of neceſſity, be ſmaller than half the focal diſtance.

Having remarked, then, that in order to magnify 100 times, the aperture of the objective lens muſt exceed 4 inches, it follows, that the focal diſtance muſt exceed 8 inches; I ſhall preſently demonſtrate that the double is not ſufficient, and that the focal diſtance of this lens muſt be increaſed beyond 300 inches. The diſtinctneſs of the expreſſion of the image requires this great increaſe, as ſhall afterwards be ſhewn; I ſatisfy myſelf with remarking at preſent, that with regard to the geometrical figure of the lens, the aperture cannot be greater than half it's focal diſtance.

Here, therefore, I ſhall go ſomewhat more into the detail, reſpecting the opening of the objective, which every magnifying power requires: and I remark, firſt, that though a ſufficient degree of clearneſs requires an opening of four inches, when the teleſcope is to magnify 100 times, we ſatisfy ourſelves, in aſtronomical inſtruments, with one of three inches, the diminution of clearneſs being ſcarcely perceptible. Hence artiſts have laid it down as a rule, that, in order to magnify 100 times, the opening of the objective lens muſt be three inches, and for other magnifying powers in that proportion. Thus, in order to magnify 50 times, it is ſufficient that the opening of the objective lens be an inch and a half; to magnify 25 times, three quarters of an inch ſuffice, and ſo of other powers.

Hence we ſee that for ſmall magnifying powers, a very ſmall opening of the objective lens is ſufficient, and that, conſequently, a moderate focal diſtance may anſwer. But if you wiſhed to magnify 200 times, the opening of the objective muſt be ſix inches, or half a foot, which requires a very large lens, whoſe focal diſtance muſt exceed even 100 feet, in order to obtain a diſtinct and exact expreſſion. For this reaſon, great magnifying powers require very long teleſcopes, at leaſt, according to the uſual arrangement of lenſes which I have explained. But, for ſome time paſt, artiſts have been ſucceſsfully employing

themſelves

themſelves in diminiſhing this exceſſive length. The opening of the objective, however, muſt follow the rule laid down, as clearneſs neceſſarily depends on it.

Were you deſirous, therefore, of conſtructing a teleſcope which ſhould magnify 400 times, the opening of the objective lens muſt be twelve inches, or a foot, let the focal diſtance be rendered as ſmall as you will: and if you wiſhed to magnify 4,000 times, the opening of the objective muſt be ten feet, a very great ſize indeed, and too much ſo for any artiſt to execute; and this is the principal reaſon, why we can never hope to carry the magnifying power ſo far, unleſs ſome great prince would be at the expenſe of providing and executing lenſes of ſuch magnitude; and, after all, perhaps, they would not ſucceed.

A teleſcope, however, which ſhould magnify 4,000 times, would diſcover many wonderful things in the heavens. The moon would appear 4,000 times larger than to the naked eye; in other words, we ſhould ſee her as if ſhe were 4,000 times nearer to us than ſhe is. Let us enquire, then, to what a degree we might be able to diſtinguiſh the different bodies which ſhe may contain. The diſtance of the moon from the earth is calculated to be 52,000 German miles;* the 4,000th part of which is 13 miles: ſuch a teleſcope would, accordingly, ſhew us the moon as if ſhe were only 13 miles diſtant; and, conſequently, we ſhould be enabled to diſcover in her the ſame things which we diſtinguiſh in objects removed to the ſame diſtance. Now, from the top of a mountain, we can eaſily diſcern other mountains more than 13 miles diſtant. There can be no doubt, then, that, with ſuch an inſtrument, we ſhould diſcover on the ſurface of the moon many things to fill us with ſurprize. But, in order to determine whether the moon is inhabited by creatures ſimilar to thoſe of the earth, a diſtance of 13 miles is ſtill too great; we muſt have, in order to this effect, a teleſcope which ſhould magnify ten times more, that is 40,000 times, and this would require an objective lens of 100 feet aperture, an enterprize which human art will never be able to execute. But, with ſuch an inſtrument, we ſhould ſee the moon as if ſhe were no farther diſtant than from Berlin to Spandau, and good eyes might eaſily diſcern men at this diſtance, if any there were, but too indiſtinctly, it muſt be allowed, to be completely aſſured of the fact.

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* For the proportion of theſe to meaſurement in Engliſh miles, ſee Vol. I. Letter I. page 3.

As we muſt reſt ſatisfied with wiſhing, on this ſubject, mine ſhould be to have at once a teleſcope which ſhould magnify 100,000 times;* the moon would then appear as if ſhe were only half a mile diſtant. The aperture of the objective lens of this teleſcope muſt be 250 feet, and we ſhould ſee, at leaſt, the larger animals which may be in the moon.

6th March, 1762.

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* Dr. Herſchel's teleſcopes actually magnify 6,500 times.

LETTER XCVIII.

eſs in the Expreſſion: On the Space of Dif- aſioned by the Aperture of Objective Lenſes, lered as the firſt Source of Want of Diſtinct- n e Repreſentation,

DISTINCTNESS of expreſſion is a quality of ſo much importance in the conſtruction of teleſcopes, that it ſeems to take precedence of all the others which I have been endeavouring to explain; for it muſt be allowed, that a teleſcope which does not repreſent diſtinctly the images of objects muſt be very defective. I muſt, therefore, unfold the reaſons of this want of diſtinctneſs, that we may apply more ſucceſsfully to the means of remedying it.

They appear ſo much the more abſtruſe, that the principles hitherto laid down do not diſcover the ſource: in fact, this defect is thus to be accounted for, one of the principles on which I have hitherto proceeded is not ſtrictly true, though not far from the truth.

You will recollect that it has been laid down as a principle, that a convex lens collects into one point of the image all the rays which come from one point of the object. Were this ſtrictly true, images repreſented by lenſes would be as diſtinctly expreſſed as the object itſelf, and we ſhould be under no apprehenſion of defect in regard to this.

Here, then, lies the defectiveneſs of this principle;

lenſes have the property now aſcribed to them only around their centre; the rays which paſs through the extremities of a lens collect in a different point from thoſe which paſs toward the centre, though all proceed from the ſame point of the object; hence are produced two different images, which occaſion indiſtinctneſs.

In order to ſet this in the cleareſt light, let us conſider the convex lens PP, *(plate IX. fig. 3.)* on the axis of which is placed the object E *e*, of which the point E, ſituated upon the axis, emits the rays EN, EM, EA, EM, EN, to the ſurface of the lens. To the direction of theſe rays, as changed by refraction, we muſt now pay attention.

I. The ray EA, which paſſes through the centre A of the lens, undergoes no refraction, but proceeds forward in the ſame direction, on the ſtraight line ABF.

II. The rays EM and EM, which are neareſt to the firſt, undergo a ſmall refraction, by which they will meet with the axis ſomewhere at F, which is the place of the image F*f*, as has been explained in ſome of my preceding letters on this ſubject.

III. The rays EN and EN, which are more remote from the axis EA, and which paſs toward the extremities NN of the lens, undergo a refraction ſomewhat different, which collects them, not at the point F, but at another point G nearer the lens, and theſe rays repreſent another image G*g*, different from the firſt F*f*.

IV. Let us now carefully attend to this particular circumſtance, not hitherto remarked; it is this, that the rays paſſing through the lens, toward it's extremities, repreſent another image G *g*, than what is repreſented by thoſe paſſing near the centre M A M.

V. If the rays E N, E N, were to retire ſtill farther from the centre A, and to paſs through the points P P of the lens, their point of re-union would be ſtill nearer to the lens, and would form a new image, nearer than even G *g*.

VI. Hence you will eaſily perceive, that the firſt image F *f*, which is named the principal image, is formed only by the rays which are almoſt infinitely near the centre; and that according as the rays retire from it, toward the extremities of the lens, a particular image is formed nearer the lens, till thoſe paſſing cloſe to the extremities form the laſt, G *g*.

VII. All the rays, therefore, which paſs through the lens P P repreſent an infinity of images diſpoſed between F *f* and G *g*; and at every diſtance from the axis the refraction of the lens produces a particular image, ſo that the whole ſpace between F and G is filled with a ſeries of images.

VIII. This ſeries of images is accordingly denominated the diffuſion of the image; and when all theſe rays afterwards enter into any eye, it is natural that the viſion ſhould be ſo much diſturbed, as the ſpace F G, through which the image is diffuſed, is more conſiderable. If this ſpace F G could be reduced to nothing, no confuſion or indiſtinctneſs need to be apprehended.

IX. The greater portions of their reſpective circles

that the arches PAP and PBP are, the greater likewiſe is FG the ſpace of diffuſion. You ſee a good reaſon, then, for rejecting all lenſes of too great thickneſs, or in which the arches which form the ſurfaces of the lens are conſiderable ſegments of their circles; (as in *plate IX. fig.* 4.) of which the arches PAP and PBP are the fourth part of the whole circumference, ſo that each contains 90 degrees; this would, conſequently, produce an inſufferable confuſion.

X. The arches, then, which form the ſurfaces of a lens, muſt contain much leſs than 90 degrees: if they contained ſo much as 60, the diffuſion of the image would be even then inſupportable. Authors who have treated the ſubject, admit of 30 degrees at moſt: and ſome fix the boundary at 20 degrees. A lens of this laſt deſcription is repreſented by *fig.* 5. of *plate IX.* in which the arches PAP and PBP contain only 20 degrees, each being but the eighteenth part of the whole circumference of it's reſpective circle.

XI. But if this lens were to ſupply the place of the objective in a teleſcope, the arches PAP and PBP muſt contain ſtill many degrees leſs. For, though the diffuſion of the image be perceptible of itſelf, the magnifying power multiplies it as many times as the object is. Therefore, the greater the magnifying power propoſed, the fewer muſt be the number of degrees, which the ſurfaces of the lens contain.

XII. When the teleſcope is intended to magnify 100 times, you will recollect, that the aperture of

the objective lens muſt be 3 inches, and it's focal diſtance 360 inches, which is equal to the radii with which the two arches PAP and PBP are deſcribed; hence it follows that each of theſe two arches contains but half a degree; and it is diſtinctneſs of expreſſion which requires an arch ſo ſmall. If it were intended to magnify 200 times, half a degree would be ſtill too much, and the meaſure of the arch, in that caſe, ought not to exceed the third part of a degree. This arch, however, muſt receive an extent of 6 inches; the radius of the circle muſt, therefore, be ſo much greater, and conſequently, alſo the focal diſtance. This is the true reaſon why great magnifying powers require teleſcopes of ſuch conſiderable length.

9th March, 1762.

LETTER XCIX.

Diminution of the Aperture of Lenſes, and other Means of leſſening the Space of Diffuſion, till it is reduced to nothing.

WHEN the ſpace of an objective lens is too great to admit of diſtinctneſs of expreſſion, it may be very eaſily remedied: you have only to cover the lens with a circle of paſteboard, leaving an opening in the centre, ſo that the lens may tranſmit no other rays, but thoſe which fall upon it through the opening, and that thoſe which before paſſed through the extremities of the lens may be excluded;

cluded; for as no rays are tranſmitted but through the middle of the lens, the ſmaller the opening is, the ſmaller likewiſe will be the ſpace of diffuſion. Accordingly, by a gradual diminution of the opening, the ſpace of diffuſion may be reduced at pleaſure.

Here the caſe is the ſame as if the lens were no larger than the opening in the paſteboard, thus the covered part becomes uſeleſs, and the opening determines the ſize of the lens; this then is the remedy employed, to give objective lenſes any given extent.

PP is the objective lens (*plate IX. fig.* 6.) before which is placed the paſteboard NN, having the opening MM, which is now the extent of the lens. This opening MM is here nearly the half of what it would be, were the paſteboard removed; the ſpace of diffuſion is, therefore, much ſmaller. It is remarked, that the ſpace of diffuſion, in this caſe, is only the fourth part of what it was before. An opening MM, reduced to a third of PP, would render the ſpace of diffuſion nine times leſs. Thus the effect of this remedy is very conſiderable; and on covering the extremities of the lens ever ſo little, the effect of it becomes perceptible.

If, therefore, a teleſcope labours under this defect, that it does not repreſent objects ſufficiently diſtinct, as a ſeries of images blended together muſt of neceſſity produce confuſion, you have only to contract the aperture of the objective lens by a covering of paſteboard ſuch as I have deſcribed, and this confuſion will infallibly diſappear. But a defect equally

embarraſſing is the conſequence, the degree of brightneſs is diminiſhed. You will recollect that every degree of the magnifying power requires a certain aperture of the objective lens, that as many rays may be tranſmitted as are neceſſary to procure a ſufficient illumination. It is vexatious, therefore, in curing one defect, to fall into another; and in order to the conſtruction of a very good teleſcope, it is abſolutely neceſſary that there ſhould be ſufficient brightneſs of illumination, without injuring diſtinctneſs in the repreſentation.

But can there be no method of diminiſhing, nay of totally reducing, the ſpace of diffuſion of objective lenſes, without diminiſhing the aperture? This is the great enquiry which has for ſome time paſt engaged the attention of the ingenious, and the ſolution of which promiſes ſuch a field of diſcovery in the ſcience of dioptricks. I ſhall have the honour, at leaſt, of laying before you, the means which ſcientific men have ſuggeſted for this purpoſe.

As the focus of the rays which paſs through the middle of a convex lens is more diſtant from the lens, than the focus of the rays which paſs through the extremities, it has been remarked that concave lenſes produce a contrary effect. This has ſuggeſted the enquiry, whether it might not be poſſible to combine a convex with a concave lens in ſuch a manner, that the ſpace of diffuſion ſhould be entirely annihilated; while, in other reſpects, this compound lens ſhould produce the ſame effect as an ordinary ſimple

objective? You know that concave lenſes are meaſured by their focal diſtance as well as thoſe which are convex; with this difference, that the focus of the concave is only imaginary, and falls before the lens, whereas the focus of convex lenſes is real, and falls behind them. Having made this remark, we reaſon as follows:

I. If we place (*plate IX. fig.* 7.) behind a convex lens P A P, a concave one Q B Q of the ſame focal diſtance, the rays which the convex lens would collect in it's focus will be refracted by the concave, ſo that they will again become parallel to each other, as they were before paſſing through the convex lens.

II. In this caſe, therefore, the concave lens deſtroys the effect of the convex, and it is the ſame thing as if the rays had proceeded in their natural direction, without undergoing any refraction. For the concave lens having it's focus at the ſame point F, reſtores the paralleliſm of the rays, which would otherwiſe have met at the point F.

III. If the focal diſtance of the concave lens were ſmaller than that of the convex, it would produce a greater effect, and would render the rays divergent, as in *fig.* 8. of *plate IX:* the incident parallel rays L M, E A, L M, paſſing through the two lenſes, would aſſume the directions N O, B F, N O, which are divergent from each other. Theſe two lenſes together produce, therefore, the ſame effect as a ſimple concave lens, which would impreſs on the incident parallel rays the ſame divergence. Two ſuch lenſes joined together, of which the concave has a ſmaller

a ſmaller focal diſtance than the convex, are therefore equivalent to a ſimple concave lens.

IV. But if the concave lens Q Q (*plate IX. fig.* 9.) has a greater focal diſtance than the convex lens P P, it is not even ſufficient to render parallel to each other the rays which the convex lens by itſelf would collect in it's focus F: theſe rays, therefore, continue convergent, but their convergence will be diminiſhed by the concave lens, ſo that the rays, inſtead of meeting in the point F, will meet in the more diſtant point O.

V. Theſe two lenſes joined together will produce, then, the ſame effect, as a ſimple convex lens which ſhould have it's focus at O, as it would collect the parallel rays L M, E A, L M, equally in the ſame point. It is therefore evident that two lenſes may be combined an infinite variety of ways, the one being convex and the other concave, ſo that their combination ſhall be equivalent to a given convex lens.

VI. Such a double objective lens may, therefore, be employed in the conſtruction of teleſcopes, inſtead of the ſimple to which it is equivalent; and the effect, as to the magnifying power, will be juſt the ſame. But the ſpace of diffuſion will be quite different, and it may happen to be greater or leſs than that of a ſimple objective, and in this laſt caſe, the double objective will be greatly preferable to the ſimple.

VII. But farther, it has been found poſſible to arrange two ſuch lenſes ſo that the ſpace of diffuſion is reduced abſolutely to nothing, which is, undoubtedly,

colour is attached to a certain number of vibrations. But ſuppoſing that this explanation ſhould ſtill appear doubtful, it is beyond all doubt, that rays of different colours likewiſe undergo different refractions in their paſſage from one tranſparent medium to another; thus red rays undergo the leaſt refraction, and violet the greateſt, though the difference is almoſt imperceptible. Now, all the other colours, as orange, yellow, green, and blue, are contained, with reſpect to refraction, within theſe two limits. It muſt likewiſe be remarked that white is a mixture of all the colours, which, by refraction, are ſeparated from each other.

In fact, when (*plate IX. fig.* 12.) a white ray O P, or a ray of the ſun, falls obliquely on a piece of glaſs A B C D, inſtead of purſuing it's courſe in the direction P Q, it not only deviates from this, but divides into a variety of rays P *r*, P *s*, P *t*, P *v*: the firſt of which P *r*, the one that deviates leaſt, repreſents the red colour, and the laſt P *v*, which deviates moſt, the violet colour. The diſperſion *r v* is indeed much ſmaller than it appears in the figure; the divergence, however, always becomes more perceptible.

From this different refrangibility of rays, according to their different colours, are produced the following phenomena, with reſpect to dioptrick glaſſes.

I. Let P P (*plate IX. fig.* 13.) be a convex lens, on the axis of which O R, at a very great diſtance A O, is the object O *o*, the image of which, as repreſented by the lens, we are to determine, putting aſide, here, the firſt irregularity, that which reſpects diffuſion, or, which

ſtruct teleſcopes incomparably ſhorter, yet poſſeſſing the ſame magnifying power.

When, employing a ſingle objective, you want to magnify a hundred times, the focal diſtance cannot be leſs than thirty feet, and the length of the teleſcope becomes ſtill greater on account of the ocular lens, whoſe focal diſtance muſt be added; a ſmall objective would produce, from it's greater ſpace of diffuſion, an intolerable confuſion. But, a length of thirty feet is not only very incommodious, but artiſts ſeldom ſucceed in forming lenſes of ſo great a focal diſtance. You will readily perceive the reaſon of this: for the radius of the ſurfaces of ſuch a lens muſt likewiſe be thirty feet, and it is very difficult to deſcribe exactly ſo great a circle, and the ſlighteſt aberration renders all the labour uſeleſs.

Accidents of this ſort are not to be apprehended in the conſtruction of compound objective lenſes, which may be formed of ſmaller circles, provided they are ſuſceptible of the aperture which the magnifying power requires. Thus, in order to magnify one hundred times, we have ſeen that the aperture of the objective lens muſt be three inches; but it would be eaſy to conſtruct a compound objective whoſe focal diſtance ſhould be only one hundred inches, and which could admit an aperture of more than three inches: therefore, as the focal diſtance of the ocular muſt be one hundred times ſmaller, it would be one inch; and the interval between the lenſes [illegible] n of their focal diſtances, the [illegible] ld be only one hundred

and one inches, or eight feet five inches, which is far ſhort of thirty feet.

But it appears to me, that a compound objective, whoſe focal diſtance ſhould be fifty inches, might eaſily admit an aperture of three inches, and even more: taking then an ocular of half an inch focus, you will obtain the ſame magnifying power of one hundred times, and the length of the teleſcope will be reduced one half, that is to four feet and leſs than three inches. Such a teleſcope, then, would produce the ſame effect as a common one of thirty feet, which is aſſuredly carrying it as far as need to be wiſhed.

If ſuch a compound objective could be made to anſwer, you would only have to double all theſe meaſurements in order to have one which ſhould admit an aperture of ſix inches, and this might be employed to magnify two hundred times, making uſe of an ocular of half an inch focus, as the two hundredth part of the focal diſtance of the objective, which would, in this caſe, be one hundred inches. Now, a common teleſcope which ſhould magnify two hundred times, muſt exceed one hundred feet in length; whereas this one, which is conſtructed with a compound objective, is reduced to about eight feet, and is perfectly accommodated to uſe, whereas a teleſcope of one hundred feet long would be an unwieldy and almoſt uſeleſs load.

The ſubject might be carried ſtill much farther, and by again doubling the meaſurements, we might have a compound objective whoſe focal diſtance ſhould be two hundred inches, or ſixteen feet eight

inches, which ſhould admit of an aperture of twelve inches or one foot: taking, then, an ocular of half an inch focus, as two hundred inches contain four hundred half inches, we ſhould have a teleſcope capable of magnifying four hundred times, and ſtill abundantly manageable, being under ſeventeen feet; whereas were we to attempt to produce the ſame magnifying power with a ſimple objective lens, the length of the teleſcope muſt exceed three hundred feet, and conſequently could be of no manner of uſe, on account of that enormous ſize.

They have at Paris a teleſcope one hundred and twenty feet long, and one at London of one hundred and thirty feet; but the dreadful trouble of mounting, and pointing them to the object, almoſt annihilates the advantages expected from them. From this you will conclude of what importance it would be to ſucceed in the conſtruction of the compound lenſes which I have been deſcribing. I ſuggeſted the firſt idea of them ſeveral years ago, and ſince then, artiſts of the greateſt ability in England and France have been attempting to execute them. Repeated efforts, and ſingular ſkill, in the artiſt, are undoubtedly requiſite. Indeed, I have made, with the aſſiſtance of an able mechanician of our Academy, ſome not unſucceſsful attempts, but the expence attending ſuch an enterprize has obliged me to give it up.

But the Royal Society of London laſt year announced, that an eminent artiſt, of the name of Dollond, had fortunately ſucceeded; and his teleſcopes are now univerſally admired. An able artiſt of Paris, named Paſſament, boaſts of a ſimilar ſucceſs. Both theſe gentlemen did me the honour, ſome time ago, to correſpond with me on the ſubject; but as the point in queſtion was chiefly, how to ſurmount certain great difficulties in the practical part, which I never attempted, it is but fair that I ſhould relinquiſh to them the honour of the diſcovery. The theory alone is my province, and it has coſt me much profound reſearch, and many painful calculations, the very ſight of which would terrify you. I ſhall therefore take care not to perplex you farther with this abſtruſe enquiry.

16th March, 1762.

LETTER CI.

Formation of Simple Objective Lenſes.

IN order to give you ſome idea of the reſearches which led me to the conſtruction of compound objective lenſes, I muſt begin with the formation of the ſimple lens.

Obſerve, firſt, that the two ſurfaces of a lens may be formed in an infinity of different ways, by taking circles of which the ſurfaces are ſegments, either equal, or unequal to each other, the focal diſtance, however, remaining always the ſame.

The ſame figure is uſually given to both ſurfaces of a lens, or, as the ſurfaces of a lens are repreſented by arches of a circle, both ſurfaces are formed with radii equal to each other. Facility of execution has, circumſtance, not hitherto remarked; it is this, that the rays paſſing through the lens, toward it's extremities, repreſent another image G *g*, than what is repreſented by thoſe paſſing near the centre M A M.

V. If the rays E N, E N, were to retire ſtill farther from the centre A, and to paſs through the points P P of the lens, their point of re-union would be ſtill nearer to the lens, and would form a new image, nearer than even G *g*.

VI. Hence you will eaſily perceive, that the firſt image F *f*, which is named the principal image, is formed only by the rays which are almoſt infinitely near the centre; and that according as the rays retire from it, toward the extremities of the lens, a particular image is formed nearer the lens, till thoſe paſſing cloſe to the extremities form the laſt, G *g*.

VII. All the rays, therefore, which paſs through the lens P P repreſent an infinity of images diſpoſed between F *f* and G *g*; and at every diſtance from the axis the refraction of the lens produces a particular image, ſo that the whole ſpace between F and G is filled with a ſeries of images.

VIII. This ſeries of images is accordingly denominated the diffuſion of the image; and when all theſe rays afterwards enter into any eye, it is natural that the viſion ſhould be ſo much diſturbed, as the ſpace F G, through which the image is diffuſed, is more conſiderable. If this ſpace F G could be reduced to nothing, no confuſion or indiſtinctneſs need to be apprehended.

IX. The greater portions of their reſpective circles

that the arches PAP and PBP are, the greater likewiſe is FG the ſpace of diffuſion. You ſee a good reaſon, then, for rejecting all lenſes of too great thickneſs, or in which the arches which form the ſurfaces of the lens are conſiderable ſegments of their circles; (as in *plate IX. fig.* 4.) of which the arches PAP and PBP are the fourth part of the whole circumference, ſo that each contains 90 degrees; this would, conſequently, produce an inſufferable confuſion.

X. The arches, then, which form the ſurfaces of a lens, muſt contain much leſs than 90 degrees: if they contained ſo much as 60, the diffuſion of the image would be even then inſupportable. Authors who have treated the ſubject, admit of 30 degrees at moſt: and ſome fix the boundary at 20 degrees. A lens of this laſt deſcription is repreſented by *fig.* 5. of *plate IX.* in which the arches PAP and PBP contain only 20 degrees, each being but the eighteenth part of the whole circumference of it's reſpective circle.

XI. But if this lens were to ſupply the place of the objective in a teleſcope, the arches PAP and PBP muſt contain ſtill many degrees leſs. For, though the diffuſion of the image be perceptible of itſelf, the magnifying power multiplies it as many times as the object is. Therefore, the greater the magnifying power propoſed, the fewer muſt be the number of degrees, which the ſurfaces of the lens contain.

XII. When the teleſcope is intended to magnify 100 times, you will recollect, that the aperture of

the objective lens muſt be 3 inches, and it's focal diſtance 360 inches, which is equal to the radii with which the two arches PAP and PBP are deſcribed; hence it follows that each of theſe two arches contains but half a degree; and it is diſtinctneſs of expreſſion which requires an arch ſo ſmall. If it were intended to magnify 200 times, half a degree would be ſtill too much, and the meaſure of the arch, in that caſe, ought not to exceed the third part of a degree. This arch, however, muſt receive an extent of 6 inches; the radius of the circle muſt, therefore, be ſo much greater, and conſequently, alſo the focal diſtance. This is the true reaſon why great magnifying powers require teleſcopes of ſuch conſiderable length.

9th March, 1762.

LETTER XCIX.

Diminution of the Aperture of Lenſes, and other Means of leſſening the Space of Diffuſion, till it is reduced to nothing.

WHEN the ſpace of an objective lens is too great to admit of diſtinctneſs of expreſſion, it may be very eaſily remedied: you have only to cover the lens with a circle of paſteboard, leaving an opening in the centre, ſo that the lens may tranſmit no other rays, but thoſe which fall upon it through the opening, and that thoſe which before paſſed through the extremities of the lens may be excluded;

cluded; for as no rays are tranſmitted but through the middle of the lens, the ſmaller the opening is, the ſmaller likewiſe will be the ſpace of diffuſion. Accordingly, by a gradual diminution of the opening, the ſpace of diffuſion may be reduced at pleaſure.

Here the caſe is the ſame as if the lens were no larger than the opening in the paſteboard, thus the covered part becomes uſeleſs, and the opening determines the ſize of the lens; this then is the remedy employed, to give objective lenſes any given extent.

PP is the objective lens (*plate IX. fig.* 6.) before which is placed the paſteboard NN, having the opening MM, which is now the extent of the lens. This opening MM is here nearly the half of what it would be, were the paſteboard removed; the ſpace of diffuſion is, therefore, much ſmaller. It is remarked, that the ſpace of diffuſion, in this caſe, is only the fourth part of what it was before. An opening MM, reduced to a third of PP, would render the ſpace of diffuſion nine times leſs. Thus the effect of this remedy is very conſiderable; and on covering the extremities of the lens ever ſo little, the effect of it becomes perceptible.

If, therefore, a teleſcope labours under this defect, that it does not repreſent objects ſufficiently diſtinct, as a ſeries of images blended together muſt of neceſſity produce confuſion, you have only to contract the aperture of the objective lens by a covering of paſteboard ſuch as I have deſcribed, and this confuſion will infallibly diſappear. But a defect equally

embarraſſing is the conſequence, the degree of brightneſs is diminiſhed. You will recollect that every degree of the magnifying power requires a certain aperture of the objective lens, that as many rays may be tranſmitted as are neceſſary to procure a ſufficient illumination. It is vexatious, therefore, in curing one defect, to fall into another; and in order to the conſtruction of a very good teleſcope, it is abſolutely neceſſary that there ſhould be ſufficient brightneſs of illumination, without injuring diſtinctneſs in the repreſentation.

But can there be no method of diminiſhing, nay of totally reducing, the ſpace of diffuſion of objective lenſes, without diminiſhing the aperture? This is the great enquiry which has for ſome time paſt engaged the attention of the ingenious, and the ſolution of which promiſes ſuch a field of diſcovery in the ſcience of dioptricks. I ſhall have the honour, at leaſt, of laying before you, the means which ſcientific men have ſuggeſted for this purpoſe.

As the focus of the rays which paſs through the middle of a convex lens is more diſtant from the lens, than the focus of the rays which paſs through the extremities, it has been remarked that concave lenſes produce a contrary effect. This has ſuggeſted the enquiry, whether it might not be poſſible to combine a convex with a concave lens in ſuch a manner, that the ſpace of diffuſion ſhould be entirely annihilated; while, in other reſpects, this compound lens ſhould produce the ſame effect as an ordinary ſimple

objective? You know that concave lenſes are meaſured by their focal diſtance as well as thoſe which are convex; with this difference, that the focus of the concave is only imaginary, and falls before the lens, whereas the focus of convex lenſes is real, and falls behind them. Having made this remark, we reaſon as follows:

I. If we place (*plate IX. fig.* 7.) behind a convex lens P A P, a concave one Q B Q of the ſame focal diſtance, the rays which the convex lens would collect in it's focus will be refracted by the concave, ſo that they will again become parallel to each other, as they were before paſſing through the convex lens.

II. In this caſe, therefore, the concave lens deſtroys the effect of the convex, and it is the ſame thing as if the rays had proceeded in their natural direction, without undergoing any refraction. For the concave lens having it's focus at the ſame point F, reſtores the paralleliſm of the rays, which would otherwiſe have met at the point F.

III. If the focal diſtance of the concave lens were ſmaller than that of the convex, it would produce a greater effect, and would render the rays divergent, as in *fig.* 8. of *plate IX:* the incident parallel rays L M, E A, L M, paſſing through the two lenſes, would aſſume the directions N O, B F, N O, which are divergent from each other. Theſe two lenſes together produce, therefore, the ſame effect as a ſimple concave lens, which would impreſs on the incident parallel rays the ſame divergence. Two ſuch lenſes joined together, of which the concave has a ſmaller

[illegible] ocal diſtance than the convex, are there- [illegible] lent to a ſimple concave lens.

[illegible] if the concave lens Q Q (*plate IX. fig.* 9.) a [illegible] er focal diſtance than the convex lens P P, it [illegible] not [illegible] ven ſufficient to render parallel to each oth [illegible] rays which the convex lens by itſelf would c [illegible] t in it's focus F: theſe rays, therefore, continue co [illegible], but their convergence will be diminiſhed [illegible] conc [illegible] len [illegible] rays, inſtead of meet- [illegible] the point [illegible] me [illegible] t in the more diſtant [illegible]

V. Th [illegible] two lenſes join [illegible] together will produce, then, [illegible] ſame effect, as a ſimple convex lens which [illegible] have it's focus at O, as it would collect the [illegible] rays L M, E A, L M, equally in the ſame [illegible]. It is therefore evident that two lenſes may be combined an infinite variety of ways, the one being convex and the other concave, ſo that their combination ſhall be equivalent to a given convex lens.

VI. Such a double objective lens may, therefore, be employed in the conſtruction of teleſcopes, inſtead of the ſimple to which it is equivalent; and the effect, as to the magnifying power, will be juſt the ſame. But the ſpace of diffuſion will be quite different, and it may happen to be greater or leſs than that of a ſimple objective, and in this laſt caſe, the double objective will be greatly preferable to the ſimple.

VII. But farther, it has been found poſſible to arrange two ſuch lenſes ſo that the ſpace of diffuſion is reduced abſolutely to nothing, which is, undoubtedly,

edly, the greateſt advantage poſſible in the conſtruction of teleſcopes. Calculation enables us to determine this arrangement, but no artiſt has hitherto been found capable of reducing it to practice.

13th March, 1762.

LETTER C.

Of Compound Objective Lenſes.

THE combination of two lenſes, of which I have now given the idea, is denominated a compound objective: the end propoſed from them is, that all the rays, as well as thoſe which paſs through the extremities of a lens, as thoſe which paſs through the middle, ſhould be collected in a ſingle point, ſo that only one image may be formed, without diffuſion, as in ſimple objectives. Could artiſts ſucceed in effecting ſuch a conſtruction, very great advantages would reſult from it, as you ſhall ſee.

It is evident, firſt, that the repreſentation of objects muſt be much more diſtinct, and more exactly expreſſed, as viſion is not diſturbed by the apparition of that ſeries of images which occupy the ſpace of diffuſion, when the objective is ſimple.

Again, as this ſpace of diffuſion is the only reaſon which obliges us to give to ſimple objectives ſuch an exceſſive focal diſtance, in order to render the inconvenience reſulting from it imperceptible; by employing compound objectives we are relieved from that cumberſome expedient, and are enabled to conſtruct

ſtruct teleſcopes incomparably ſhorter, yet poſſeſſing the ſame magnifying power.

When, employing a ſingle objective, you want to magnify a hundred times, the focal diſtance cannot be leſs than thirty feet, and the length of the teleſcope becomes ſtill greater on account of the ocular lens, whoſe focal diſtance muſt be added; a ſmall objective would produce, from it's greater ſpace of diffuſion, an intolerable confuſion. But, a length of thirty feet is not only very incommodious, but artiſts ſeldom ſucceed in forming lenſes of ſo great a focal diſtance. You will readily perceive the reaſon of this: for the radius of the ſurfaces of ſuch a lens muſt likewiſe be thirty feet, and it is very difficult to deſcribe exactly ſo great a circle, and the ſlighteſt aberration renders all the labour uſeleſs.

Accidents of this ſort are not to be apprehended in the conſtruction of compound objective lenſes, which may be formed of ſmaller circles, provided they are ſuſceptible of the aperture which the magnifying power requires. Thus, in order to magnify one hundred times, we have ſeen that the aperture of the objective lens muſt be three inches; but it would be eaſy to conſtruct a compound objective whoſe focal diſtance ſhould be only one hundred inches, and which could admit an aperture of more than three inches: therefore, as the focal diſtance of the ocular muſt be one hundred times ſmaller, it would be one inch; and the interval between the lenſes being the ſum of their focal diſtances, the length of the teleſcope would be only one hundred

and one inches, or eight feet five inches, which is far ſhort of thirty feet.

But it appears to me, that a compound objective, whoſe focal diſtance ſhould be fifty inches, might eaſily admit an aperture of three inches, and even more: taking then an ocular of half an inch focus, you will obtain the ſame magnifying power of one hundred times, and the length of the teleſcope will be reduced one half, that is to four feet and leſs than three inches. Such a teleſcope, then, would produce the ſame effect as a common one of thirty feet, which is aſſuredly carrying it as far as need to be wiſhed.

If ſuch a compound objective could be made to anſwer, you would only have to double all theſe meaſurements in order to have one which ſhould admit an aperture of ſix inches, and this might be employed to magnify two hundred times, making uſe of an ocular of half an inch focus, as the two hundredth part of the focal diſtance of the objective, which would, in this caſe, be one hundred inches. Now, a common teleſcope which ſhould magnify two hundred times, muſt exceed one hundred feet in length; whereas this one, which is conſtructed with a compound objective, is reduced to about eight feet, and is perfectly accommodated to uſe, whereas a teleſcope of one hundred feet long would be an unwieldy and almoſt uſeleſs load.

The ſubject might be carried ſtill much farther, and by again doubling the meaſurements, we might have a compound objective whoſe focal diſtance ſhould be two hundred inches, or ſixteen feet eight

circumſtance, not hitherto remarked; it is this, that the rays paſſing through the lens, toward it's extremities, repreſent another image G g, than what is repreſented by thoſe paſſing near the centre MAM.

V. If the rays EN, EN, were to retire ſtill farther from the centre A, and to paſs through the points PP of the lens, their point of re-union would be ſtill nearer to the lens, and would form a new image, nearer than even G g.

VI. Hence you will eaſily perceive, that the firſt image F f, which is named the principal image, is formed only by the rays which are almoſt infinitely near the centre; and that according as the rays retire from it, toward the extremities of the lens, a particular image is formed nearer the lens, till thoſe paſſing cloſe to the extremities form the laſt, G g.

VII. All the rays, therefore, which paſs through the lens PP repreſent an infinity of images diſpoſed between F f and G g; and at every diſtance from the axis the refraction of the lens produces a particular image, ſo that the whole ſpace between F and G is filled with a ſeries of images.

VIII. This ſeries of images is accordingly denominated the diffuſion of the image; and when all theſe rays afterwards enter into any eye, it is natural that the viſion ſhould be ſo much diſturbed, as the ſpace FG, through which the image is diffuſed, is more conſiderable. If this ſpace FG could be reduced to nothing, no confuſion or indiſtinctneſs need to be apprehended.

IX. The greater portions of their reſpective circles

that the arches PAP and PBP are, the greater likewiſe is FG the ſpace of diffuſion. You ſee a good reaſon, then, for rejecting all lenſes of too great thickneſs, or in which the arches which form the ſurfaces of the lens are conſiderable ſegments of their circles; (as in *plate IX. fig.* 4.) of which the arches PAP and PBP are the fourth part of the whole circumference, ſo that each contains 90 degrees; this would, conſequently, produce an inſufferable confuſion.

X. The arches, then, which form the ſurfaces of a lens, muſt contain much leſs than 90 degrees: if they contained ſo much as 60, the diffuſion of the image would be even then inſupportable. Authors who have treated the ſubject, admit of 30 degrees at moſt: and ſome fix the boundary at 20 degrees. A lens of this laſt deſcription is repreſented by *fig.* 5. of *plate IX.* in which the arches PAP and PBP contain only 20 degrees, each being but the eighteenth part of the whole circumference of it's reſpective circle.

XI. But if this lens were to ſupply the place of the objective in a teleſcope, the arches PAP and PBP muſt contain ſtill many degrees leſs. For, though the diffuſion of the image be perceptible of itſelf, the magnifying power multiplies it as many times as the object is. Therefore, the greater the magnifying power propoſed, the fewer muſt be the number of degrees, which the ſurfaces of the lens contain.

XII. When the teleſcope is intended to magnify 100 times, you will recollect, that the aperture of

the objective lens muſt be 3 inches, and it's focal diſtance 360 inches, which is equal to the radii with which the two arches PAP and PBP are deſcribed; hence it follows that each of theſe two arches contains but half a degree; and it is diſtinctneſs of expreſſion which requires an arch ſo ſmall. If it were intended to magnify 200 times, half a degree would be ſtill too much, and the meaſure of the arch, in that caſe, ought not to exceed the third part of a degree. This arch, however, muſt receive an extent of 6 inches; the radius of the circle muſt, therefore, be ſo much greater, and conſequently, alſo the focal diſtance. This is the true reaſon why great magnifying powers require teleſcopes of ſuch conſiderable length.

9th March, 1762.

LETTER XCIX.

Diminution of the Aperture of Lenſes, and other Means of leſſening the Space of Diffuſion, till it is reduced to nothing.

WHEN the ſpace of an objective lens is too great to admit of diſtinctneſs of expreſſion, it may be very eaſily remedied: you have only to cover the lens with a circle of paſteboard, leaving an opening in the centre, ſo that the lens may tranſmit no other rays, but thoſe which fall upon it through the opening, and that thoſe which before paſſed through the extremities of the lens may be excluded;

cluded; for as no rays are tranſmitted but through the middle of the lens, the ſmaller the opening is, the ſmaller likewiſe will be the ſpace of diffuſion. Accordingly, by a gradual diminution of the opening, the ſpace of diffuſion may be reduced at pleaſure.

Here the caſe is the ſame as if the lens were no larger than the opening in the paſteboard, thus the covered part becomes uſeleſs, and the opening determines the ſize of the lens; this then is the remedy employed, to give objective lenſes any given extent.

PP is the objective lens (*plate IX. fig.* 6.) before which is placed the paſteboard NN, having the opening MM, which is now the extent of the lens. This opening MM is here nearly the half of what it would be, were the paſteboard removed; the ſpace of diffuſion is, therefore, much ſmaller. It is remarked, that the ſpace of diffuſion, in this caſe, is only the fourth part of what it was before. An opening MM, reduced to a third of PP, would render the ſpace of diffuſion nine times leſs. Thus the effect of this remedy is very conſiderable; and on covering the extremities of the lens ever ſo little, the effect of it becomes perceptible.

If, therefore, a teleſcope labours under this defect, that it does not repreſent objects ſufficiently diſtinct, as a ſeries of images blended together muſt of neceſſity produce confuſion, you have only to contract the aperture of the objective lens by a covering of paſteboard ſuch as I have deſcribed, and this confuſion will infallibly diſappear. But a defect equally

embarraſſing is the conſequence, the degree of brightneſs is diminiſhed. You will recollect that every degree of the magnifying power requires a certain aperture of the objective lens, that as many rays may be tranſmitted as are neceſſary to procure a ſufficient illumination. It is vexatious, therefore, in curing one defect, to fall into another; and in order to the conſtruction of a very good teleſcope, it is abſolutely neceſſary that there ſhould be ſufficient brightneſs of illumination, without injuring diſtinctneſs in the repreſentation.

But can there be no method of diminiſhing, nay of totally reducing, the ſpace of diffuſion of objective lenſes, without diminiſhing the aperture? This is the great enquiry which has for ſome time paſt engaged the attention of the ingenious, and the ſolution of which promiſes ſuch a field of diſcovery in the ſcience of dioptricks. I ſhall have the honour, at leaſt, of laying before you, the means which ſcientific men have ſuggeſted for this purpoſe.

As the focus of the rays which paſs through the middle of a convex lens is more diſtant from the lens, than the focus of the rays which paſs through the extremities, it has been remarked that concave lenſes produce a contrary effect. This has ſuggeſted the enquiry, whether it might not be poſſible to combine a convex with a concave lens in ſuch a manner, that the ſpace of diffuſion ſhould be entirely annihilated; while, in other reſpects, this compound lens ſhould produce the ſame effect as an ordinary ſimple

objective? You know that concave lenſes are meaſured by their focal diſtance as well as thoſe which are convex; with this difference, that the focus of the concave is only imaginary, and falls before the lens, whereas the focus of convex lenſes is real, and falls behind them. Having made this remark, we reaſon as follows:

I. If we place *(plate IX. fig.* 7.) behind a convex lens P A P, a concave one Q B Q of the ſame focal diſtance, the rays which the convex lens would collect in it's focus will be refracted by the concave, ſo that they will again become parallel to each other, as they were before paſſing through the convex lens.

II. In this caſe, therefore, the concave lens deſtroys the effect of the convex, and it is the ſame thing as if the rays had proceeded in their natural direction, without undergoing any refraction. For the concave lens having it's focus at the ſame point F, reſtores the paralleliſm of the rays, which would otherwiſe have met at the point F.

III. If the focal diſtance of the concave lens were ſmaller than that of the convex, it would produce a greater effect, and would render the rays divergent, as in *fig.* 8. of *plate IX:* the incident parallel rays L M, E A, L M, paſſing through the two lenſes, would aſſume the directions N O, B F, N O, which are divergent from each other. Theſe two lenſes together produce, therefore, the ſame effect as a ſimple concave lens, which would impreſs on the incident parallel rays the ſame divergence. Two ſuch lenſes joined together, of which the concave has a ſmaller

a ſmall ocal diſtance than the convex, are there- lent to a ſimple concave lens.

if the concave lens Q Q (*plate IX. fig.* 9.) a t er focal diſtance than the convex lens P P, not even ſufficient to render parallel to each oth he rays which the convex lens by itſelf would collect in it's focus F: theſe rays, therefore, continue convergent, but their convergence will be diminiſhed by the concave lens, ſo that the rays, inſtead of meeting in the point F, will meet in the more diſtant point O.

V. Theſe two lenſes joined together will produce, then, the ſame effect, as a ſimple convex lens which ſhould have it's focus at O, as it would collect the parallel rays L M, E A, L M, equally in the ſame point. It is therefore evident that two lenſes may be combined an infinite variety of ways, the one being convex and the other concave, ſo that their combination ſhall be equivalent to a given convex lens.

VI. Such a double objective lens may, therefore, be employed in the conſtruction of teleſcopes, inſtead of the ſimple to which it is equivalent; and the effect, as to the magnifying power, will be juſt the ſame. But the ſpace of diffuſion will be quite different, and it may happen to be greater or leſs than that of a ſimple objective, and in this laſt caſe, the double objective will be greatly preferable to the ſimple.

VII. But farther, it has been found poſſible to arrange two ſuch lenſes ſo that the ſpace of diffuſion is reduced abſolutely to nothing, which is, undoubtedly,

mical teleſcope of the ſame magnifying power. By means, then, of theſe teleſcopes, with four lenſes, we obtain a double advantage, the object is repreſented upright, and a much larger field is diſcovered: both, circumſtances of much importance.

X. Finally, it is poſſible to find ſuch an arrangement of theſe four lenſes, as, without affecting either of the advantages now mentioned, ſhall entirely do away the defect ariſing from the colours of the rainbow, and at the ſame time repreſent the object with all poſſible diſtinctneſs. But few artiſts can attain this degree of perfection.

10th April, 1762.

LETTER CVIII.

Precautions to be obſerved in the Conſtruction of Teleſcopes. Neceſſity of blackening the Inſide of Tubes. Diaphragms.

AFTER theſe reſearches reſpecting the conſtruction of teleſcopes, I muſt ſuggeſt and explain certain precautions neceſſary to be uſed; which, though they relate neither to the lenſes themſelves, nor to their arrangement, are nevertheleſs of ſuch importance, that if they are not very carefully obſerved, the beſt inſtrument is rendered entirely uſeleſs. It is not ſufficient that the lenſes ſhould be arranged in ſuch a manner that all the rays which fall upon them ſhall be tranſmitted through theſe lenſes to the eye; care muſt be taken, beſides, to prevent ocal diſtance than the convex, are there- lent to a ſimple concave lens.

if the concave lens Q Q (*plate IX. fig.* 9.) er focal diſtance than the convex lens P P, it even ſufficient to render parallel to each othe : rays which the convex lens by itſelf would collect in it's focus F: theſe rays, therefore, continue conve nt, but their convergence will be diminiſhed by oncave lens, ſo that e rays, inſtead of meet- i the int F, will meet in the more diſtant

V. two join together will produce, then, ſame t, as a ple convex lens which e s foci as it would collect the I, equally in the ſame t that two lenſes may be combined an infinite variety of ways, the one being convex and the other concave, ſo that their combination ſhall be equivalent to a given convex lens.

VI. Such a double objective lens may, therefore, be employed in the conſtruction of teleſcopes, inſtead of the ſimple to which it is equivalent; and the effect, as to the magnifying power, will be juſt the ſame. But the ſpace of diffuſion will be quite different, and it may happen to be greater or leſs than that of a ſimple objective, and in this laſt caſe, the double objective will be greatly preferable to the ſimple.

VII. But farther, it has been found poſſible to arrange two ſuch lenſes ſo that the ſpace of diffuſion is reduced abſolutely to nothing, which is, undoubtedly,

deeply, no new rays will be produced, let the light be ever ſo ſtrong. This blackening muſt be carried through the whole length of the teleſcope, as there is no black ſo deep as not to generate, when illuminated, ſome faint light: ſuppoſing, then, that ſome extraneous rays were to make their way to the ſecond lens B, the black of the tube, purſuing their courſe, would eaſily abſorb them altogether. There is a brilliant black, which, for this reaſon, it would be very improper to employ.

V. But even this precaution is not ſufficient, it is neceſſary likewiſe to furniſh the inſide of the tube with one or more diaphragms, perforated with a ſmall circular aperture, the better to exclude all extraneous light; but care muſt be taken that they exclude not the rays of the object which the inſtrument is intended to repreſent. See *plate* X. *fig.* 4.

VI. It is neceſſary to obſerve at what place, in the tube, the proper rays of the object are moſt contracted; this muſt be at the points where their images are repreſented, for there all the rays are collected together. Now, the objective lens A repreſents the image in it's focus at M. You have only, then, to compute the magnitude of this image, and there to fix your diaphragm, whoſe aperture *m n* ſhall be equal to the magnitude of the image, or rather ſomewhat greater. For if the aperture were leſs than the image, there would be a proportional loſs of the apparent field, which is always a great defect.

VII. Theſe are the obſervations, reſpecting the

diaphragm,

ſtruct teleſcopes incomparably ſhorter, yet poſſeſſing the ſame magnifying power.

When, employing a ſingle objective, you want to magnify a hundred times, the focal diſtance cannot be leſs than thirty feet, and the length of the teleſcope becomes ſtill greater on account of the ocular lens, whoſe focal diſtance muſt be added; a ſmall objective would produce, from it's greater ſpace of diffuſion, an intolerable confuſion. But, a length of thirty feet is not only very incommodious, but artiſts ſeldom ſucceed in forming lenſes of ſo great a focal diſtance. You will readily perceive the reaſon of this: for the radius of the ſurfaces of ſuch a lens muſt likewiſe be thirty feet, and it is very difficult to deſcribe exactly ſo great a circle, and the ſlighteſt aberration renders all the labour uſeleſs.

Accidents of this ſort are not to be apprehended in the conſtruction of compound objective lenſes, which may be formed of ſmaller circles, provided they are ſuſceptible of the aperture which the magnifying power requires. Thus, in order to magnify one hundred times, we have ſeen that the aperture of the objective lens muſt be three inches; but it would be eaſy to conſtruct a compound objective whoſe focal diſtance ſhould be only one hundred inches, and which could admit an aperture of more than three inches: therefore, as the focal diſtance of the ocular muſt be one hundred times ſmaller, it would be one inch; and the interval between the lenſes being the ſum of their focal diſtances, the length of the teleſcope would be only one hundred

and one inches, or eight feet five inches, which is far ſhort of thirty feet.

But it appears to me, that a compound objective, whoſe focal diſtance ſhould be fifty inches, might eaſily admit an aperture of three inches, and even more: taking then an ocular of half an inch focus, you will obtain the ſame magnifying power of one hundred times, and the length of the teleſcope will be reduced one half, that is to four feet and leſs than three inches. Such a teleſcope, then, would produce the ſame effect as a common one of thirty feet, which is aſſuredly carrying it as far as need to be wiſhed.

If ſuch a compound objective could be made to anſwer, you would only have to double all theſe meaſurements in order to have one which ſhould admit an aperture of ſix inches, and this might be employed to magnify two hundred times, making uſe of an ocular of half an inch focus, as the two hundredth part of the focal diſtance of the objective, which would, in this caſe, be one hundred inches. Now, a common teleſcope which ſhould magnify two hundred times, muſt exceed one hundred feet in length; whereas this one, which is conſtructed with a compound objective, is reduced to about eight feet, and is perfectly accommodated to uſe, whereas a teleſcope of one hundred feet long would be an unwieldy and almoſt uſeleſs load.

The ſubject might be carried ſtill much farther, and by again doubling the meaſurements, we might have a compound objective whoſe focal diſtance ſhould be two hundred inches, or ſixteen feet eight

inches,

the almighty power, in union with the ſovereign wiſdom and goodneſs, of the Great Creator.

Thus the moſt important diſcoveries have been made reſpecting the planets, which, to the unaſſiſted eye, appear only as ſo many luminous points; but which, viewed through a good teleſcope, reſemble the moon, and appear even ſtill much greater.

But you will be not a little ſurprized, when I aſſure you, that with the aſſiſtance of the beſt teleſcope, even one which magnifies more than two hundred times, the fixed ſtars ſtill appear only as points, nay ſtill ſmaller than to the naked eye. This is ſo much the more aſtoniſhing, that it is certain the teleſcope repreſents them ſuch as they would appear were we two hundred times nearer. Are we not, hence, reduced to the neceſſity of concluding that, here, teleſcopes fail to produce their effect? But this idea preſently vaniſhes, on conſidering that they diſcover to us millions of little ſtars which, without their aid, muſt have for ever eſcaped the eye. We likewiſe perceive the diſtances between the ſtars incomparably greater; for two ſtars which, to the naked eye, ſeemed almoſt to touch each other, when viewed through the teleſcope, are ſeen at a very conſiderable diſtance; a ſufficient proof of the effect of the teleſcope.

What, then, is the reaſon that the fixed ſtars appear to us ſmaller through the teleſcope than to the naked eye? In reſolving this queſtion, I remark, firſt, that the fixed ſtars appear greater to the naked eye than they ought to do, and that this ariſes from a

that the arches PAP and PBP are, the greater likewiſe is FG the ſpace of diffuſion. You ſee a good reaſon, then, for rejecting all lenſes of too great thickneſs, or in which the arches which form the ſurfaces of the lens are conſiderable ſegments of their circles; (as in *plate IX. fig.* 4.) of which the arches PAP and PBP are the fourth part of the whole circumference, ſo that each contains 90 degrees; this would, conſequently, produce an inſufferable confuſion.

X. The arches, then, which form the ſurfaces of a lens, muſt contain much leſs than 90 degrees: if they contained ſo much as 60, the diffuſion of the image would be even then inſupportable. Authors who have treated the ſubject, admit of 30 degrees at moſt: and ſome fix the boundary at 20 degrees. A lens of this laſt deſcription is repreſented by *fig.* 5. of *plate IX.* in which the arches PAP and PBP contain only 20 degrees, each being but the eighteenth part of the whole circumference of it's reſpective circle.

XI. But if this lens were to ſupply the place of the objective in a teleſcope, the arches PAP and PBP muſt contain ſtill many degrees leſs. For, though the diffuſion of the image be perceptible of itſelf, the magnifying power multiplies it as many times as the object is. Therefore, the greater the magnifying power propoſed, the fewer muſt be the number of degrees, which the ſurfaces of the lens contain.

XII. When the teleſcope is intended to magnify 100 times, you will recollect, that the aperture of

the objective lens muſt be 3 inches, and it's focal diſtance 360 inches, which is equal to the radii with which the two arches PAP and PBP are deſcribed; hence it follows that each of theſe two arches contains but half a degree; and it is diſtinctneſs of expreſſion which requires an arch ſo ſmall. If it were intended to magnify 200 times, half a degree would be ſtill too much, and the meaſure of the arch, in that caſe, ought not to exceed the third part of a degree. This arch, however, muſt receive an extent of 6 inches; the radius of the circle muſt, therefore, be ſo much greater, and conſequently, alſo the focal diſtance. This is the true reaſon why great magnifying powers require teleſcopes of ſuch conſiderable length.

9th March, 1762.

LETTER XCIX.

Diminution of the Aperture of Lenſes, and other Means of leſſening the Space of Diffuſion, till it is reduced to nothing.

WHEN the ſpace of an objective lens is too great to admit of diſtinctneſs of expreſſion, it may be very eaſily remedied: you have only to cover the lens with a circle of paſteboard, leaving an opening in the centre, ſo that the lens may tranſmit no other rays, but thoſe which fall upon it through the opening, and that thoſe which before paſſed through the extremities of the lens may be excluded;

cluded; for as no rays are tranſmitted but through the middle of the lens, the ſmaller the opening is, the ſmaller likewiſe will be the ſpace of diffuſion. Accordingly, by a gradual diminution of the opening, the ſpace of diffuſion may be reduced at pleaſure.

Here the caſe is the ſame as if the lens were no larger than the opening in the paſteboard, thus the covered part becomes uſeleſs, and the opening determines the ſize of the lens; this then is the remedy employed, to give objective lenſes any given extent.

PP is the objective lens (*plate IX. fig.* 6.) before which is placed the paſteboard NN, having the opening MM, which is now the extent of the lens. This opening MM is here nearly the half of what it would be, were the paſteboard removed; the ſpace of diffuſion is, therefore, much ſmaller. It is remarked, that the ſpace of diffuſion, in this caſe, is only the fourth part of what it was before. An opening MM, reduced to a third of PP, would render the ſpace of diffuſion nine times leſs. Thus the effect of this remedy is very conſiderable; and on covering the extremities of the lens ever ſo little, the effect of it becomes perceptible.

If, therefore, a teleſcope labours under this defect, that it does not repreſent objects ſufficiently diſtinct, as a ſeries of images blended together muſt of neceſſity produce confuſion, you have only to contract the aperture of the objective lens by a covering of paſteboard ſuch as I have deſcribed, and this confuſion will infallibly diſappear. But a defect equally

embarraſſing is the conſequence, the degree of brightneſs is diminiſhed. You will recollect that every degree of the magnifying power requires a certain aperture of the objective lens, that as many rays may be tranſmitted as are neceſſary to procure a ſufficient illumination. It is vexatious, therefore, in curing one defect, to fall into another; and in order to the conſtruction of a very good teleſcope, it is abſolutely neceſſary that there ſhoul[illegible] be ſufficient brightneſs of illumination, without inju[illegible] diſtinctneſs in the repreſentation.

But can there be no method of diminiſhing, nay of totally reducing, the ſpace of diffuſion of objective lenſes, without diminiſhing the aperture? This is the great enquiry which has for ſome time paſt engaged the attention of the ingenious, and the ſolution of which promiſes ſuch a field of diſcovery in the ſcience of dioptricks. I ſhall have the honour, at leaſt, of laying before you, the means which ſcientific men have ſuggeſted for this purpoſe.

As the focus of the rays which paſs through the middle of a convex lens is more diſtant from the lens, than the focus of the rays which paſs through the extremities, it has been remarked that concave lenſes produce a contrary effect. This has ſuggeſted the enquiry, whether it might not be poſſible to combine a convex with a concave lens in ſuch a manner, that the ſpace of diffuſion ſhould be entirely annihilated; while, in other reſpects, this compound lens ſhould produce the ſame effect as an ordinary ſimple

objective? You know that concave lenſes are meaſured by their focal diſtance as well as thoſe which are convex; with this difference, that the focus of the concave is only imaginary, and falls before the lens, whereas the focus of convex lenſes is real, and falls behind them. Having made this remark, we reaſon as follows:

I. If we place (*plate IX. fig.* 7.) behind a convex lens P A P, a concave one Q B Q of the ſame focal diſtance, the rays which the convex lens would collect in it's focus will be refracted by the concave, ſo that they will again become parallel to each other, as they were before paſſing through the convex lens.

II. In this caſe, therefore, the concave lens deſtroys the effect of the convex, and it is the ſame thing as if the rays had proceeded in their natural direction, without undergoing any refraction. For the concave lens having it's focus at the ſame point F, reſtores the paralleliſm of the rays, which would otherwiſe have met at the point F.

III. If the focal diſtance of the concave lens were ſmaller than that of the convex, it would produce a greater effect, and would render the rays divergent, as in *fig.* 8. of *plate IX:* the incident parallel rays L M, E A, L M, paſſing through the two lenſes, would aſſume the directions N O, B F, N O, which are divergent from each other. Theſe two lenſes together produce, therefore, the ſame effect as a ſimple concave lens, which would impreſs on the incident parallel rays the ſame divergence. Two ſuch lenſes joined together, of which the concave has a ſmaller

requires. There cannot remain the ſhadow of doubt as to this; but, for that very reaſon, the difficulty, inſtead of diminiſhing, gathers ſtrength; and it will be aſked with ſo much the more eagerneſs; How comes it that the whole world agrees in imagining the moon to be greater when riſing or ſetting, though her apparent diameter is then, in reality, ſmaller? and, What can be the reaſon of this deluſion to which men are univerſally ſubject? The aſtronomer, who knows perfectly well that the moon's apparent diameter is then ſmaller, falls nevertheleſs into the ſame deception as the moſt ignorant clown.

20th April, 1762.

LETTER CXI.

Reflections on the Queſtion reſpecting the Moon's apparent Magnitude; Progreſs toward a Solution of the Difficulty. Abſurd Explanations.

YOU would ſcarcely have believed, that the ſimple appearance of the moon involved ſo many difficulties; but I hope I ſhall be able to clear the way toward a ſolution, by the following reflections.

I. It is not aſtoniſhing that our judgment reſpecting the magnitude of objects ſhould not always be in correſpondence with the viſual angle under which we ſee it: of this, daily experience furniſhes ſufficient proof. A cat, for example, appears, when very near, under a greater angle than an ox at the diſtance of 100 paces. I could never, at the ſame time, imagine the cat to be larger than the ox: and you will pleaſe to recollect, that our judgment reſpecting magnitude is always intimately connected with that of diſtance; ſo that if we commit a miſtake in the calculation of diſtance, our judgment reſpecting magnitude becomes, of neceſſity, erroneous.

II. In order to elucidate this more clearly, it ſometimes happens that a fly paſſing ſuddenly before the eye, without our thinking of it, if our ſight is fixed on a diſtant object, we imagine, at firſt, that the fly is at a great diſtance; and as it appears under a very conſiderable angle, we take it, for a moment, to be a large fowl, which, at the proper diſtance, would appear under the ſame angle. It is, then, inconteſtably certain, that our judgment reſpecting the magnitude of objects is not regulated by the viſual angle under which they are ſeen, and that there is a very great difference between the apparent magnitude of objects, and the calculated or computed magnitude. The firſt is regulated by the viſual angle, and the other depends on the diſtance to which we ſuppoſe the object to be removed.

III. To avail myſelf of this remark, I farther obſerve, that we ought not to ſay, that we ſee the moon greater in the horizon, than at a conſiderable elevation. This is abſolutely falſe, for we then ſee her even ſomewhat leſs. But to ſpeak accurately, we ought to ſay that we judge and compute the moon greater when ſhe is in the horizon; and this is literally true with the unanimous conſent of all mankind. This is ſufficient to reconcile the apparent

contradiction

ſtruct t[illegible]s incomparably ſhorter, yet poſſeſſing the ſame [illegible]ifying power.

W[illegible]en, [illegible]loying a ſingle objective, you want to m[illegible]dred times, the focal diſtance cannot b[illegible]es than thirty feet, and the length of the teleſcope becomes ſtill greater on account of the ocular lens, whoſe focal diſtance muſt be added; a ſmall objective would produce, from it's greater ſpace of diffuſion, an intolerable [illegible]atur[illegible]n. But, a length of thirty feet is not o[illegible] ve[illegible] incommodious, but artiſts ſeldom ſucceed in forming lenſes of ſo great a focal diſtance. You will readily perceive the reaſon of this: for the radius of the ſurfaces of ſuch a lens muſt likewiſe be thirty feet, and it is very difficult to deſcribe exactly ſo great a circle, and the ſlighteſt aberration renders all the labour uſeleſs.

Accidents of this ſort are not to be apprehended in the conſtruction of compound objective lenſes, which may be formed of ſmaller circles, provided they are ſuſceptible of the aperture which the magnifying power requires. Thus, in order to magnify one hundred times, we have ſeen that the aperture of the objective lens muſt be three inches; but it would be eaſy to conſtruct a compound objective whoſe focal diſtance ſhould be only one hundred inches, and which could admit an aperture of more than three inches: therefore, as the focal diſtance of the ocular muſt be one hundred times ſmaller, it would be one inch; and the interval between the lenſes being the ſum of their focal diſtances, the length of the teleſcope would be only one hundred

and one inches, or eight feet five inches, which is far ſhort of thirty feet.

But it appears to me, that a compound objective, whoſe focal diſtance ſhould be fifty inches, might eaſily admit an aperture of three inches, and even more: taking then an ocular of half an inch focus, you will obtain the ſame magnifying power of one hundred times, and the length of the teleſcope will be reduced one half, that is to four feet and leſs than three inches. Such a teleſcope, then, would produce the ſame effect as a common one of thirty feet, which is aſſuredly carrying it as far as need to be wiſhed.

If ſuch a compound objective could be made to anſwer, you would only have to double all theſe meaſurements in order to have one which ſhould admit an aperture of ſix inches, and this might be employed to magnify two hundred times, making uſe of an ocular of half an inch focus, as the two hundredth part of the focal diſtance of the objective, which would, in this caſe, be one hundred inches. Now, a common teleſcope which ſhould magnify two hundred times, muſt exceed one hundred feet in length; whereas this one, which is conſtructed with a compound objective, is reduced to about eight feet, and is perfectly accommodated to uſe, whereas a teleſcope of one hundred feet long would be an unwieldy and almoſt uſeleſs load.

The ſubject might be carried ſtill much farther, and by again doubling the meaſurements, we might have a compound objective whoſe focal diſtance ſhould be two hundred inches, or ſixteen feet eight

ſight appear, is deſtitute of ſolidity. On looking at the moon in the horizon, through a ſmall aperture made in any body which ſhall conceal the intermediate objects, ſhe nevertheleſs ſtill ſeems greater. Beſides, we do not always imagine that objects, between which and us many other bodies interpoſe, are more diſtant. A great hall, for example, when quite empty, uſually appears much larger than when filled with company, not [illegible] ıanding the numerous objects then interpoſ[illegible] een us and the walls of the apartment.

24th April, 1762.

LETTER CXII.

An Attempt toward the true Explanation of this Phenomenon: The Moon appears more diſtant when in the Horizon, than when at a great Elevation.

WE are ſtill, then, very far from the true ſolution of this univerſal illuſion, under which all, without exception, are induced to imagine the moon to be much greater when in the horizon, than when conſiderably elevated. I have already remarked, that this phenomenon is ſo much the more unaccountable, from it's being demonſtrable that the moon's apparent diameter is then even ſomewhat leſs: we ought not, therefore, to ſay, that we then
the moon greater, but that we imagine her to

:cordingly, I have very often obſerved our judgment of objects to differ very widely from viſion itſelf. We do not heſitate, for example, to conclude, that a horſe 100 paces diſtant is larger than a dog one pace diſtant, though the apparent magnitude of the dog is unqueſtionably greater, or, which amounts to the ſame thing, though the image of the dog, painted on the bottom of the eye, be greater than that of the horſe. Our judgment, in this caſe, is regulated by taking diſtance into the account, and laying it down that the horſe is much farther off than the dog, we conclude he is much larger.

It is very probable, therefore, that the ſame circumſtance may take place reſpecting the moon's appearance, and induce us to reckon the moon greater, when in the horizon, than at a conſiderable elevation. In the caſe of the horſe, our computation of diſtance was founded in truth; but here, as it is abſolutely erroneous, the illuſion muſt be ſingularly unaccountable, but muſt, at the ſame time, have a certain foundation, as it's prevalence is univerſal, and cannot, therefore, be imputed to caprice. Wherein can it conſiſt? This is to be the ſubject of our preſent enquiry.

I. Every one conſiders the azure expanſe of heaven as a flattened arch, the ſummit of which is much nearer to us than the under part, where it meets the horizon. A perſon, accordingly, ſtanding on a plane AB *(plate X. fig. 7.)* which extends as far as his ſight perceives the vault of heaven, commonly called the firmament, under the figure A E F B, in which the diſtances C A and C B are much greater than from the zenith to C.

II. This idea is likewiſe, beyond all queſtion, a mere illuſion; there being, in reality, no ſuch vault ſurrounding and incloſing us on every ſide. It is a void of immenſe extent, as it reaches to the moſt diſtant of the fixed ſtars, an interval that far exceeds all power of imagination. I uſe the word *void* to diſtinguiſh it from groſs terreſtrial bodies. For, near the earth, ſpace is occupied by our atmoſphere; and beyond, by that fluid, infinitely more ſubtile, which we call *ether*.

III. Though this vault, however, has no real exiſtence, it poſſeſſes an undoubted reality in our imagination; and all mankind, the philoſopher as well as the clown, are ſubject to the ſame illuſion. On the ſurface of this arch we imagine the ſun, the moon, and all the ſtars to be diſpoſed, like ſo many brilliant ſtuds affixed to it; and though we have a perfect conviction of the contrary, we cannot help giving into the illuſion.

IV. This being laid down, when the moon is in the horizon, imagination attaches her to the point A or B of this ſuppoſed vault, and hence we conclude her diſtance to be as much greater as we conſider the line C A or C B to be greater than C Z; but when, as ſhe aſcends, and approaches the zenith, we imagine ſhe comes nearer, and if ſhe reaches the very zenith we think ſhe is at the leaſt poſſible diſtance.

V. The illuſion, as to diſtance, neceſſarily involves that which reſpects magnitude. As the moon at A appears much farther from C, than in the zenith, we are, in a manner, forced to conclude, that the moon is really ſo much greater; and that in the ſame proportion that the diſtance C A appears to exceed the diſtance C Z. All will not, perhaps, agree in determining this proportion; one will ſay, the moon appears to him twice as great, when in the horizon; another will ſay three times, and the generality will declare for the medium between two and three; but every one will infallibly agree in aſſerting that the moon appears larger.

VI. It may be neceſſary, here, to preſent you with the demonſtration of this propoſition. The computation of magnitude is neceſſarily involved in the computation of diſtance. When the moon is near the horizon, we ſee her *(plate X. fig.* 8.*)* under a certain angle, ſay M C A, the ſpectator being at C: and, when ſhe is at a very great elevation, let N C D be the angle under which we ſee her. It is evident that theſe two angles M C A and N C D are nearly equal to each other, the difference being imperceptible.

VII. But, in the firſt caſe, as we eſtimate the moon's diſtance to be much greater, or equal to the line C A, with reference to the imaginary vault above deſcribed, it follows, that we compute the moon's diameter to be equal to the line M A. But, in the other caſe, the diſtance of the moon C D appears much ſmaller, and, conſequently, as the angle N C D is equal to the angle M C A, the computed

magnitud

magnitude DN will be much ſmaller than the computed magnitude AM.

VIII. To put this beyond a doubt, you have only to cut off from the lines CM and CA, the parts C*d* and C*n* equal to the lines CD and CN; and as in the two triangles C*dn* and CDN, the angles at the point C are equal, the triangles themſelves are likewiſe ſo, and, conſequently, the line DN will be equal to the line *dn*; but *dn* is evidently ſmaller than AM, and that, as many times as the diſtance C*d* and CD is leſs than CA. This is a clear demonſtration of the reaſon why we eſtimate the moon to be greater when in the horizon, than when near the zenith.

29th April, 1762.

LETTER CXIII.

The Heavens appear under the Form of an Arch flattened toward the Zenith.

YOU will tax me, no doubt, with pretending to explain one illuſion by another equally unaccountable. It may be ſaid, that the imaginary vault of heaven is altogether as inconceivable as the increaſed appearance of the moon and the other heavenly bodies, when in, or near, the horizon. The objection is not without foundation, and therefore lays me under the neceſſity of attempting to explain the true reaſon, why the heavens appear in the form of an arch flattened toward the ſummit. The following reflections may, perhaps, be received as an acquittance of my engagement.

I. In order to account for this imaginary vault, it will be alleged that it proceeds from the appearance of the heavenly bodies, as ſeeming more remote, when in the horizon, than when near to, or in, the zenith. This is, undoubtedly, a formal *petitio principii*, as logicians call it, or a begging of the queſtion, which every one is entitled to reject as a ground of reaſoning. In truth, having ſaid above, that the imaginary vault of heaven makes the moon, in the horizon, appear farther off than when near the zenith, it would be ridiculous to affirm, that the thing which leads us to imagine the exiſtence of ſuch a vault is, that horizontal objects appear more diſtant than vertical.

II. It was not, however, uſeleſs to ſuggeſt the idea of this imaginary vault, though it may not carry us a great way forward; and after I ſhall have explained, wherefore the heavenly bodies appear more remote when viewed near the horizon, you will be enabled to comprehend, at the ſame time, the reaſon of that two-fold univerſal illuſion, namely, the apparently increaſed magnitude of the heavenly bodies, when in the horizon, and the flattened arch of heaven.

III. The whole, then, reverts to this, to explain wherefore the heavenly bodies, when ſeen in the horizon, appear more remote than when at a conſiderable elevation: I now affirm, it is becauſe theſe objects appear leſs brilliant; and this impoſes on me which we ſee in, or near, our horizon, have a longer ſpace of the atmoſphere to pervade, than when nearer the zenith. When this is done, you will eaſily comprehend, why the heavenly bodies appear much leſs brilliant when near the horizon, than at the time of riſing and ſetting. This ſhall be the ſubject of my next letter.

1ſt May, 1762.

LETTER CXIV.

Reaſon aſſigned for the Faintneſs of the Light of Heavenly Bodies in the Horizon.

WHAT I have juſt advanced, namely, that the rays of the heavenly bodies, when in the horizon, have a larger portion of our atmoſphere to pervade, may appear ſomewhat paradoxical, conſidering that the atmoſphere univerſally extends to the ſame height, ſo that, at whatever point the ſtar may be, it's rays muſt always penetrate through the whole of that height, before it can reach our eyes. The following reflections, I flatter myſelf, will give you complete ſatisfaction on the ſubject.

I. It is, firſt of all, neceſſary to form a juſt idea of the atmoſphere which ſurrounds our globe. For this purpoſe, the interior circle A B C D (*plate* X. *fig*. 10.) ſhall repreſent the earth, and the exterior dotted circle *a b c d* ſhall mark the height of the atmoſphere. Let it be remarked that, univerſally, in proportion as the air riſes above the ſurface of the earth, continually loaded with heterogeneous particles, riſing into it above the earth, ſuch as vapours and exhalations, which deſtroy it's tranſparency; ſo that if a ray ſhould fall in with ſuch a particle, it would be intercepted, and almoſt extinguiſhed by it. It is accordingly evident, that the more the air is loaded with ſuch particles, which prevent the tranſmiſſion of light, the more rays muſt be loſt by the interception; and you know that a very thick miſt deprives the air of almoſt all it's tranſparency, to ſuch a degree, that it is frequently impoſſible to diſtinguiſh objects at three paces diſtance.

VII. Let the points marked, in *plate X. fig.* 9. repreſent ſuch particles, ſcattered through the air, whoſe number is greater or leſs, according as the air is more or leſs tranſparent. It is evident, that many of the rays, which pervade that ſpace, muſt be loſt, and that the loſs muſt be greater, in proportion as the ſpace which they had to run through that air is greater. We ſee, then, that diſtant objects become inviſible in a fog, while ſuch as are very near the eye, may be ſtill perceptible, becauſe the rays of the firſt meet, in their progreſs, a greater number of particles which obſtruct their tranſmiſſion.

VIII. We muſt hence conclude, that the longer the ſpace is, through which the rays of the heavenly bodies have to paſs through the atmoſphere, in order to reach our eyes, the more conſiderable muſt be their loſs or diminution. Of this you can no longer entertain any doubt. All that remains, then, is ſimply to demonſtrate, that the rays of the ſtars which we ſee in, or near, our horizon, have a longer ſpace of the atmoſphere to pervade, than when nearer the zenith. When this is done, you will eaſily comprehend, why the heavenly bodies appear much leſs brilliant when near the horizon, than at the time of riſing and ſetting. This ſhall be the ſubject of my next letter.

1ſt May, 1762.

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I. It is, firſt of all, neceſſary to form a juſt idea of the atmoſphere which ſurrounds our globe. For this purpoſe, the interior circle A B C D *(plate X. fig.* 10.*)* ſhall repreſent the earth, and the exterior dotted circle *a b c d* ſhall mark the height of the atmoſphere. Let it be remarked that, univerſally, in proportion as the air riſes above the ſurface of the earth, it becomes always more tranſparent and ſubtile, ſo that, at laſt, it is imperceptibly loſt in the ether, which fills the whole expanſe of heaven.

II. The groſſer air, that which is moſt loaded with the particles that intercept and extinguiſh the rays of light, is univerſally found in the lower regions, near the ſurface of the earth. It becomes, therefore, more ſubtile as we aſcend, and leſs obſtructive of the light; and, at the height of a German mile, has become ſo tranſparent, as to occaſion no perceptible obſtrúction whatever, of the light. The diſtance, then, between the interior circle and the exterior, may be fixed at a German mile, nearly, whereas the ſemi-diameter of the globe contains about 860 of ſuch miles: ſo that the height of the atmoſphere is a very ſmall matter, compared with the magnitude of the globe.

III. Let us now conſider, (*plate X. fig.* 11.) a ſpectator at A, on the ſurface of the earth; and drawing from the centre of the globe G, through A, the line G Z, it will be directed toward the zenith of the ſpectator. The line A S, which is perpendicular, and touches the earth, will be horizontal to it. Conſequently, he will ſee a ſtar at Z in his zenith, or in the ſummit of the heavens; but a ſtar at S will appear to him in the horizon, at it's riſing or ſetting. Each of theſe ſtars may be conſidered as infinitely diſtant from the earth, though it was impoſſible to repreſent this in the figure.

IV. Now you have only to caſt your eye once more on the figure, to be ſatisfied that the rays proceeding from S have a much longer ſpace to travel through the atmoſphere, than thoſe from the ſtar Z, before they reach the ſpectator at A. Thoſe from the ſtar Z have only to paſs through the perpendicular height of the atmoſphere *a* A, which is not above a German mile; whereas thoſe that come from the ſtar S have to travel the whole ſpace *b* A, which is evidently much longer; and could the figure be repreſented more conformably to the fact, ſo as to exhibit the radius G A 860 times longer than the height A *a*, we ſhould find the diſtance A *b* to exceed 40 ſuch miles.

V. It is farther of importance to remark, that the rays of the ſtar Z have but a very ſmall ſpace to travel through the lower region of the atmoſphere, which is moſt loaded with vapour; whereas the rays of the ſtar S have a much longer courſe to perform through that region, and are obliged to crawl, if I may uſe the expreſſion, along the ſurface of the earth. The concluſion, then, is obvious. The rays of the ſtar Z undergo ſcarcely any diminution of luſtre, but thoſe of the ſtar S muſt be almoſt extinguiſhed from ſo long a paſſage through the groſſer air.

VI. It is indiſputably certain, then, that the ſtars which we ſee in the horizon, muſt appear with a luſtre extremely diminiſhed; and it will ſimply account to you for a well-known fact, that you can, without any inconvenience, fix your eyes ſteadily on the riſing or ſetting ſun, whereas at noon, or at a conſiderable elevation, his luſtre is inſupportable. This is the firſt point I undertook to demonſtrate; I proceed to the ſecond, namely, to prove that it is the diminution of light which forces us, almoſt, to imagine the heavenly bodies at a much greater diſtance, than when we ſee them in all their luſtre.

VII. The reaſon muſt be ſought in terreſtrial bodies, with which we are every day converſant, and reſpecting whoſe diſtance we form a judgment. But for the ſame reaſon that rays of light, in paſſing through the air, undergo ſome diminution of luſtre, it is evident, that the farther an object is removed from us, the more of it's luſtre it loſes, and the more obſcure it becomes in proportion. Thus a very diſtant mountain appears quite dark; but, on a nearer approach, we can eaſily diſcover trees on it, and other minuter objects, which it was impoſſible to diſtinguiſh at a very remote diſtance.

VIII. This obſervation, ſo general, and which never miſleads us in contemplating terreſtrial bodies, has produced in us, from our childhood, this fundamental principle, from which we conclude objects to be diſtant in proportion as the rays of light which they emit are weakened. It is in virtue of this principle, therefore, that we conclude the moon to be farther off at riſing and ſetting, than at a conſiderable elevation; and for the ſame reaſon we conclude ſhe is ſo much greater. You will, I flatter myſelf, admit this reaſoning to be ſolid; and this embarraſſing phenomenon to be as clearly elucidated as the nature of the ſubject permits.

4th May, 1762.

LETTER CXV.

Illuſion reſpecting the Diſtance of Objects, and the Diminution of Luſtre.

THE principle of our imagination, by which I have endeavoured to explain the phenomenon of the moon's greater apparent magnitude in the horizon than at a conſiderable elevation, is ſo deeply rooted in our nature as to become the ſource of a thouſand ſimilar illuſions, ſome of which I will take the liberty to ſuggeſt.

We have been habituated from infancy, almoſt involuntarily, to imagine objects to be diſtant in proportion as their luſtre is diminiſhed: and, on the other hand, very brilliant objects appear to be nearer than they really are. This illuſion can proceed only from an ill-regulated imagination, which very frequently miſleads us. It is nevertheleſs ſo natural, and ſo univerſal, that no one is capable of guarding againſt it, though the error, in many caſes, is extremely palpable, as I have ſhewed in the inſtance of the moon: but we are equally deceived in a variety of other inſtances, As I ſhall preſently make appear.

I. It is a well-known illuſion, that the flame of a conflagration, in the night, appears much nearer than it really is. The reaſon is obvious; the fire blazes in all it's luſtre, and in conformity to a principle pre-eſtabliſhed in the imagination, we always conclude it to be nearer than it is in reality.

II. For the ſame reaſon, a great hall, the walls of which are perfectly white, always appears ſmaller. White, you know, is the moſt brilliant colour: hence we conclude the walls of ſuch an apartment to be too near, and, conſequently, the apparent magnitude is thereby diminiſhed.

III. But in an apartment hung with black, as is the cuſtom in mournings, we perceive the directly oppoſite effect. The apartment now appears conſiderably more ſpacious than it really is. Black is, undeniably, the moſt gloomy of colours, for it reflects ſcarcely any light on the eye; hence the walls of an apartment in deep mourning ſeem more diſtant than they are, and conſequently greater; but let the black hangings be removed, and the white colour re-appear, and the apartment will ſeem contracted.

IV. No claſs of men avail themſelves more of this natural and univerſal illuſion than painters. The ſame picture, you know, repreſents ſome objects as at a great diſtance, and others as very near; and here the ſkill of the artiſt is moſt conſpicuous. It is not a little ſurprizing, that though we know, to abſolute certainty, all the repreſentations of a picture to be expreſſed on the ſame ſurface, and, conſequently, at nearly the ſame diſtance from the eye, we ſhould be, nevertheleſs, under the power of illuſion, and imagine ſome to be quite near, and others extremely diſtant. This illuſion is commonly aſcribed to a dextrous management of light and ſhade; which, undoubtedly, furniſh the painter with endleſs reſources.

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ſources. But you have only to look at a picture to be ſenſible, that the objects intended to be thrown to a great diſtance, are but faintly and even indiſtinctly expreſſed. Thus, when the eye is directed to very remote objects, we eaſily perceive, for example, that they are men, but it is impoſſible to diſtinguiſh the parts, ſuch as the eyes, the noſe, the mouth; and it is in conformity to this appearance, that the painter repreſents objects. But thoſe which he intends ſhould appear cloſe to us, he diſplays in all the brightneſs of colouring, and is at pains clearly to expreſs each minute particular. If they are perſons, we can diſtinguiſh the ſmalleſt lineaments of the face, the folds of the drapery, &c.: this part of the repreſentation ſeems, I may ſay, to riſe out of the canvas, while other parts appear to ſink and retire.

V. On this illuſion, therefore, the whole art of painting entirely reſts. Were we accuſtomed to form our judgment in ſtrict conformity to truth, this art would make no more impreſſion on us than if we were blind. To no purpoſe would the painter call forth all his powers of genius, and employ the happieſt arrangement of colours, we ſhould coldly affirm, on that piece of canvas, there is a red ſpot, here a blue one; there a black ſtroke, here ſome whitiſh lines: every thing is on the ſame plane ſurface; there is no riſing nor ſinking; therefore no real object can be repreſented in this manner: the whole would, in this caſe, be conſidered as a ſcrawling on paper, and we ſhould, perhaps, fatigue ourſelves to no purpoſe, in attempting to decypher the meaning of all theſe different coloured ſpots. Would not a man, in ſuch a ſtate of perfection, be an object of much compaſſion, thus deprived of the pleaſure reſulting from the productions of an art, at once ſo amuſing, and ſo inſtructive?

8th May, 1762.

LETTER CXVI.

On the Azure Colour of the Heavens.

YOU are now enabled to comprehend the reaſon why the ſun and moon appear much greater when in the horizon, than at a conſiderable elevation. It conſiſts in this, that we then unintentionally compute theſe bodies to be at a greater diſtance, a computation founded on the very conſiderable diminution which their luſtre, in that poſition, undergoes, from the longer paſſage which the rays have to force, through the lower region of the atmoſphere, which is the moſt loaded with vapours and exhalations, whereby the tranſparency is diminiſhed. This is a brief recapitulation of the reflections which I have taken the liberty to ſuggeſt on this ſubject.

This quality of the air, which diminiſhes tranſparency, might, at firſt ſight, be conſidered as a defect. But on attending to conſequences, we ſhall find it ſo far from being ſuch, that we ought, on the contrary, to acknowledge in it the infinite wiſdom and goodneſs of the CREATOR. To this impurity of the air we are indebted for that wonderful and rav ſpectacle which the azure of the heavens preſents to the eye; for the opaque particles, which obſtruct the rays of light, are illuminated by them, and afterwards re-tranſmit their own proper rays, produced in their ſurface by a violent agitation, as is the caſe in all opaque bodies. Now, it is the number of vibrations communicated to them, which repreſents to us this magnificent azure: a circumſtance which well deſerves to be completely unfolded.

I. I obſerve, firſt, that theſe particles are extremely minute and conſiderably diſtant from each other, beſides their being delicately fine and almoſt wholly tranſparent. Hence it comes to paſs, that each, ſeparately, is abſolutely imperceptible, ſo that we can be affected by them only when a very great number tranſmit their rays at once to the eye, and nearly in the ſame direction. The rays of ſeveral muſt, therefore, be collected, in order to excite a ſenſation.

II. Hence it clearly follows, that ſuch of theſe particles as are near to us eſcape our ſenſes, for they muſt be conſidered as points diſperſed through the maſs of air.

But ſuch as are very diſtant from the eye, as (*plate XI. fig.* 1.) the points *a b c* collect in the eye, almoſt according to the ſame direction, their ſeveral rays, which, thus, become ſufficiently ſtrong to affect the ſight, eſpecially when it is conſidered that ſimilar particles more remote, *e f g h*, as well as others more near, concur in producing this effect.

III. The azure colour which we ſee in the heavens, when ſerene, is nothing elſe, then, but the reſult of all theſe particles diſperſed through the atmoſphere, eſpecially of ſuch as are very remote: it may be affirmed, therefore, that they are in their nature blue, but a blue extremely clear, which becomes not ſufficiently deep and perceptible, except when they are in a very great number, and unite their rays according to the ſame direction.

IV. Art has the power of producing a ſimilar effect. If, on diſſolving a ſmall quantity of indigo, in a great quantity of water, you let that water fall drop by drop, you will not perceive in the ſeparate drops the ſlighteſt appearance of colour; and on pouring ſome of it into a ſmall goblet, you will perceive only a faint bluiſh colour. But if you fill a large veſſel with the ſame water, and view it at a diſtance, you will perceive a very deep blue. The ſame experiment may be made with other colours. Burgundy wine, in very ſmall quantities, appears only to be faintly reddiſh, but in a large flaſk completely filled, the wine appears of a deep red.

V. Water, in a large and deep veſſel, preſents ſomething like colour, but, in a ſmall quantity, is altogether clear and limpid. This colour is, commonly, more or leſs of a greeniſh caſt, which may warrant us in ſaying, that the minute particles of water are likewiſe ſo, but of a colour ſo delicately fine that a great maſs of it muſt be collected, before the colour can be perceptible, becauſe the rays of a multitude of particles then concur toward producing this effect.

VI. As it appears probable, from this obſervation, that the minute particles of water are greeniſh, it might be maintained, that the reaſon why the ſea, or the water of a lake or pool, appears green, is the very ſame that gives the heavens the appearance of azure. For it is more probable, that all the particles of the air ſhould have a faintly bluiſh caſt, but ſo very faint, as to be imperceptible till preſented in a prodigious maſs, ſuch as the whole extent of the atmoſphere; than that this colour is to be aſcribed to vapours floating in the air, but which do not appertain to it.

VII. In fact, the purer the air is, and the more purged from exhalation, the brighter is the luſtre of heaven's azure; which is a ſufficient proof, that we muſt look for the reaſon of it, in the nature of the proper particles of the air. Extraneous ſubſtances mingling with it, ſuch as exhalations, become, on the contrary, injurious to that beautiful azure, and ſerve to diminiſh it's luſtre. When the air is overloaded with ſuch vapours, they produce fogs near the ſurface, and entirely conceal from us the azure appearance; when they are more elevated, as is frequently the caſe, they form clouds, which frequently cover the whole face of the ſky, and preſent a very different colour from that of this azure of the pure air. This, then, is a new quality of air, different from thoſe formerly explained, ſubtility, fluidity and elaſticity; namely, the minute particles of air, are in their nature bluiſh.

11th May, 1762.

LETTER CXVII.

What the Appearance would be, were the Air perfectly tranſparent.

INDEPENDENT of the beautiful ſpectacle of the azure heavens, procured for us by this colour of the circumambient air, we ſhould be miſerable in the extreme, were it perfectly tranſparent, and diveſted of thoſe bluiſh particles; and we have here a new reaſon for adoring the infinite wiſdom and goodneſs of the CREATOR.

That you may have full conviction of the truth of my aſſertion, let us ſuppoſe the air to be quite tranſparent, and ſimilar to the ether, which, we know, tranſmits all the rays of the ſtars, without intercepting ſo much as one, and contains no particles themſelves illuminated by rays, for ſuch a particle could not be ſo, without intercepting ſome of the rays which fell upon it. If the air were in this ſtate, the rays of the ſun would paſs freely through it, without the re-tranſmiſſion of any light to the eye: we ſhould receive, then, thoſe rays only which came to us immediately from the ſun. The whole heavens, except the ſpot occupied by the ſun, would appear, therefore, completely dark; and, inſtead of this brilliant blue, we ſhould diſcover nothing, on looking upward, but the deepeſt black and the moſt profound night.

Plate XI. fig. 2, repreſents the ſun; and the point than that of the ſun; and this will convince you, that the light proceeding from the ſtars, is a mere nothing in compariſon with the light of the ſun. But the illumination of the heavens, in the day-time, even though the ſun ſhould be over-clouded, is ſo great, as many thouſand times to exceed the light of the full m

You muſt have frequently perceived that, in the
t, when the ſtars appear much
, ar ly of ſuperior magni-
are t e moon's vicinity; a
nt pr nger light always ab-
the feebler.

It is, then, an unſpeakable benefit, that our atmoſphere begins to be illuminated by the ſun, even before he riſes, as we are thereby prepared to bear the vivacity of his rays, which would otherwiſe be inſupportable, that is, if the tranſition from night to day were inſtantaneous. The ſeaſon during which the atmoſphere is gradually illuminated before ſun-riſing, and continues to be illuminated after he ſets, is denominated twilight. This ſubject, from it's importance, merits a particular explanation, which I propoſe to attempt in my next Letter: and thus, one article in phyſics naturally runs into another.

15th May, 1762.

LETTER

ſtar N, however near it might be to the ſun. It is eaſy to comprehend in what a diſmal ſtate we ſhould then be. This proximity of luſtre inſupportable, and darkneſs the moſt profound, muſt deſtroy the organs of viſion, and quickly reduce us to total blindneſs. Of this ſome judgment may be formed from the inconvenience we feel on paſſing ſuddenly from darkneſs into light.

Now, this dreadful inconvenience is completely remedied by the nature of the air, from it's containing particles opaque to a very ſmall degree, and ſuſceptible of illumination. Accordingly, the moment the ſun is above the horizon, nay ſomewhat earlier, the whole atmoſphere becomes illuminated with his rays, and we are preſented with that beautiful azure which I have deſcribed, ſo that our eyes, whichever way directed, receive a great quantity of rays, generated in the ſame particles. Thus, on looking toward M, *(ſee the ſame figure as before)* we perceive a great degree of light, produced by this brilliant azure of the heavens.

This very illumination of the atmoſphere prevents our ſeeing the ſtars by day: the reaſon of this is obvious. It far exceeds that of the ſtars, and the greater light always makes the leſſer to diſappear; and the nerves of the retina, at the bottom of the eye, being already ſtruck by a very ſtrong light, are no longer ſenſible to the impreſſion made by the feebler light of the ſtars.

You will pleaſe to recollect that the light of the full moon is upward of 300,000 times more faint our horizon begins to be illuminated at R, and in proportion as the ſun approaches the horizon, a greater part of it will be illuminated, till it becomes, at length, completely luminous.

This re[illegible]ion leads me forward to another phenomenon equally intereſting, and very intimately connected with it, nam[illegible] that the atmoſphere diſcovers to us the [illegible] ſun and of the other ſtars, ſome time [illegible] above the horizon, and ſome time after [illegible]e fallen below it, by means of the r[illegible] rays of light undergo on paſſing from the [illegible] into the groſſer air, which conſtitutes our atmoſphere; of this I proceed to give you the demonſtration.

I. Rays of light do not continue to proceed forward in a ſtraight line any longer than they move through a tranſparent medium of the ſame nature. As ſoon as they paſs from one medium to another, they are diverted from their rectilinear direction, their path is as it were broken off; and this is what we call refraction, which I formerly explained at conſiderable length, and demonſtrated that rays, on paſſing from air into glaſs, and reciprocally, are thus broken or refracted:

II. Now air being a different medium from ether, when a ray of light paſſes from ether into air, it muſt, of neceſſity, undergo ſome refraction.

Thus, the arch of the circle A M B (*plate XI. fig.* 4.) terminating our upper atmoſphere, if a ray of light M S, from the ether, falls upon it at M, it will not proceed ſtraight forward in the ſame direction

LETTER CXVIII.

Refraction of Rays of Light in the Atmoſphere, and it's Effects. Of the Twilight. Of the apparent riſing and ſetting of the Heavenly Bodies.

IN order to explain the cauſe of the twilight, or that illumination of the heavens which precedes the riſing of the ſun, and continues ſome time after he is ſet, I muſt refer you to what has been already demonſtrated reſpecting the horizon and the atmoſphere.

Let the circle A O B D (*plate XI. fig.* 3.) repreſent the earth, and the dotted circle *a o b d* the atmoſphere: let a point O be aſſumed on the ſurface of the earth, through which draw the ſtraight line H O R I, touching the earth at O, and this line H I will repreſent the horizon, which ſeparates that part of the heavens which is viſible to us, from that which is not. As ſoon as the ſun has reached this line, he appears in the horizon, both at riſing and ſetting, and the whole atmoſphere is then completely illuminated. But let us ſuppoſe the ſun, before his riſing, to be ſtill under the horizontal line at S: from which the ray S T R, grazing the earth at T, may reach the point of the atmoſphere, ſituated in our horizon; the opaque particles which are there will already be illuminated by that ray, and conſequently have become viſible. Accordingly, ſome time before the riſing of the ſun, the atmoſphere *b o* R over low the horizon at S, or, which is the ſame thing, below the horizontal line O M V.

V. However, as the ray M O, which meets the eye, is horizontal, we aſſign that direction to the ſun himſelf, and imagine him to be actually at V, that is, in the horizon, though he is ſtill below it. And reciprocally, as often as we ſee the ſun, or any other ſtar, in the horizon, we are aſſured they are ſtill below it, according to the angle S M V, which aſtronomers have obſerved to be about half a degree, or, more exactly, 32 minutes.

VI. In the morning, then, we ſee the ſun before he has reached our horizon, that is, while he is yet an angle of 32 minutes below it; and in the evening a conſiderable time after he is really ſet, as we ſee him till he has deſcended an angle of 32 minutes. We call that the true riſing and ſetting of the ſun, when he is actually in the horizon; and the commencement of his appearance in the morning, and diſappearing at night, we denominate the apparent riſing and ſetting.

VII. This refraction of the atmoſphere, which renders the apparent riſing and ſetting of the ſun both earlier and later than the real, procures for us the benefit of a much longer day than we ſhould enjoy, did not the atmoſphere produce this effect. Such is the explanation of a very important phenomenon in nature.

18th May, 1762.

tion M N, but will aſſume, on entering into the air, the direction M R, ſomewhat different from M N, and the angle N M R is denominated the angle of refraction, or, ſimply, the refraction.

III. I have already remarked, that the refraction is greater in proportion as the ray S M falls more obliquely on the ſurface of the atmoſphere; or as the angle BMS is ſmaller or more acute. For if the ray SM falls perpendicularly on the ſurface of the atmoſphere, that is, if the angle B M S is a right angle, no refraction will take place, but the ray will purſue it's progreſs in the ſame ſtraight line. This rule is univerſally applicable to every kind of refraction, whatever may be the nature of the two media, through which the rays travel.

IV. Let the arch of the circle A O B *(plate XI. fig.* 5.) repreſent the ſurface of the earth, and the arch E M F terminate the atmoſphere. If you draw at O the line O M V, touching the ſurface of the earth at O, it will be horizontal. And if the ſun is ſtill under the horizon at S, ſo as to be ſtill inviſible (for no one of his rays can yet reach us in a ſtraight line) the ray S M, being continued in a ſtraight line, would paſs over us to N; but as it falls on the atmoſphere at M, and in a very oblique direction, the angle F M S being very acute, it will thence undergo a very conſiderable refraction; and inſtead of proceeding forward to N, would aſſume the direction M O, ſo that the ſun would be actually viſible to a perſon at O, though ſtill conſiderably below there be a ſtar at S: were there no atmoſphere, the ray SMO would proceed in a ſtraight line to the eye at O, and we ſhould ſee it in the ſame direction OMS, where it would actually be, that is, we ſhould ſee it in it's true place. Let us, then, meaſure the angle SOR, formed by the ray SO with the horizon OR, and this a ned the height of the ſtar, or it's el : horizon. We meaſure alſo the med by the ray SO with the verti inating in the zenith: and as the angl is a ght angle, or 90 degrees, we have o the angle SOZ from 90 degrees, to have SOR, which gives the true elevation of the ſtar.

III. But let us now attend to the atmoſphere, which I ſuppoſe terminated by the arch HDNMR; and, I remark, firſt, that the preceding ray SM of the ſtar S, on entering into M in the atmoſphere, does not proceed directly forward to the eye at O, but, from the refraction, will aſſume another direction as MP, and conſequently will not meet the eye at O: ſo that if this ſtar ſent down to the earth only that ray SM, to a perſon at O it would be abſolutely inviſible. But it muſt be conſidered, that every luminous point emits it's rays in all directions, and that all ſpace is filled with them.

IV. There will be, then, among others, ſome ray, as SN, which is broken or refracted, on entering the atmoſphere at N: ſo that it's continuation NO ſhall paſs preciſely to an eye at O. The refracted ray NO is not, therefore, in a ſtraight line with the

ray

LETTER CXIX.

The Stars appear at a greater Elevation than they are. Table of Refractions.

YOU have now, no doubt, a clear idea of this ſingular effect of our atmoſphere, by which the ſun and the other heavenly bodies are rendered viſible in the horizon, though conſiderably below it, whereas they would be inviſible but for the refraction. For the ſame reaſon, the ſun, and all the heavenly bodies, always appear at a greater elevation above the horizon than they really are. It is neceſſary, therefore, carefully to diſtinguiſh the apparent elevation of a ſtar, from what it would be were there no atmoſphere. I ſhall endeavour to ſet this in the cleareſt light poſſible.

I. Let the arch A O B *(plate XI. fig. 6.)* be part of the ſurface of the earth, and O the ſpot where we are, through which draw the ſtraight line HOR, touching the ſurface, and this line HOR will repreſent the true horizon. From O let there be drawn perpendicularly the ſtraight line O Z, which is the ſame thing as ſuſpending a given weight by a cord. This line is ſaid to be vertical, and the point Z of the heavens, in which it terminates, is called the zenith. This line O Z, then, is perpendicular to the horizontal H O R, ſo that one being known, the other muſt be known likewiſe.

II. This being laid down, let *(plate XI. fig. 7.)* there differs not from the S N *s*, and which we call the refraction. For, though the angle S N *s*, as being the external angle to the triangle S N O, is equal to the two internal and oppoſite angles taken together, namely, S O N and N S O, we may conſider, on account of the immenſe diſtance of the ſtars, the lines O S and N S as parallel, and, conſequently, the angle O S N vaniſhes; ſo that the angle S O N is nearly equal to the angle of refraction S N *s*.

VIII. Having found, then, the apparent altitude of a ſtar, you muſt ſubtract from it the refraction, in order to have the real altitude, which there is no other method of diſcovering. For this purpoſe, aſtronomers have been at much pains to aſcertain the refraction to be ſubtracted from each apparent altitude, that is, to determine how much muſt be abated in order to reduce the apparent to the real altitude.

IX. From a long ſeries of obſervations, they have been, at length, enabled to conſtruct a table, called the table of refraction, in which is marked for every apparent altitude, the refraction, or angle to be ſubtracted. Thus, when the apparent altitude is nothing, that is, when the ſtar appears in the horizon, the refraction is 32 minutes, the ſtar is, accordingly, an angle of actually 32 minutes below the horizon. But if the ſtar has acquired any degree of elevation, be it ever ſo inconſiderable, the refraction becomes much leſs. At the altitude of 15 degrees it is no more than four minutes; at the altitude of 40 degrees it is only one minute; and as the altitude encreaſes, the refraction always becomes leſs, till, at length ray S M; and if N O be produced forward to *s*, the continuation N *s* will form an angle with the ray NS, namely, the angle S N *s*, which is what we call the refraction, and which is greater in proportion as the angle S N R, under which the ray S N enters into the atmoſphere, is more acute, as was demonſtrated in the preceding Letter.

V. It is the ray N O, conſequently, which paints in the eye the image of the ſtar S, and which renders it viſible: and as this ray comes in the direction NO, as if the ſtar were in it, we imagine the ſtar likewiſe to be ſituated in the direction N O, or in that line continued, ſomewhere at *s*. This point *s* being different from the real place of the ſtar S, we call *s* the apparent place of the ſtar, which muſt be carefully diſtinguiſhed from it's place S, where the ſtar would be ſeen, were there no atmoſphere.

VI. Since, then, the ſtar is ſeen by the ray N O, the angle N O R, which this ray N O makes with the horizon, is the apparent altitude of the ſtar; and when, by a proper inſtrument, we meaſure the angle N O R, we are ſaid to have found the apparent altitude of the ſtar; the real altitude being, as we have ſhewed, the angle R O S.

VII. Hence it is evident, that the apparent altitude RON, is greater than the real altitude ROM, ſo that the ſtars appear to us at a greater elevation above the horizon than they really are, for the ſame reaſon they appear already in the horizon while they are ſtill below it. Now the exceſs of the apparent altitude above the true, is the angle M O N, which differs not from the S N *s*, and which we call the refraction. For, though the angle S N *s*, as being the external angle to the triangle S N O, is equal to the two internal and oppoſite angles taken together, namely, S O N and N S O, we may conſider, on account of the immenſe diſtance of the ſtars, the lines O S and N S as parallel, and, conſequently, the angle O S N vaniſhes; ſo that the angle S O N is nearly equal to the angle of refraction S N *s*.

VIII. Having found, then, the apparent altitude of a ſtar, you muſt ſubtract from it the refraction, in order to have the real altitude, which there is no other method of diſcovering. For this purpoſe, aſtronomers have been at much pains to aſcertain the refraction to be ſubtracted from each apparent altitude, that is, to determine how much muſt be abated in order to reduce the apparent to the real altitude.

IX. From a long ſeries of obſervations, they have been, at length, enabled to conſtruct a table, called the table of refraction, in which is marked for every apparent altitude, the refraction, or angle to be ſubtracted. Thus, when the apparent altitude is nothing, that is, when the ſtar appears in the horizon, the refraction is 32 minutes, the ſtar is, accordingly, an angle of actually 32 minutes below the horizon. But if the ſtar has acquired any degree of elevation, be it ever ſo inconſiderable, the refraction becomes much leſs. At the altitude of 15 degrees it is no more than four minutes; at the altitude of 40 degrees it is only one minute; and as the altitude encreaſes, the refraction always becomes leſs, till, at length, it entirely diſappears, at the altitude of 90 degrees.

X. This is the caſe when a ſtar is ſeen in the very zenith; for it's elevation is then 90 degrees, and the real and apparent altitude is the ſame: and we are fully aſſured that a ſtar ſeen in the zenith is actually there, and that the refraction of the atmoſphere does not change it's place, as at every other degree of altitude.

THE END.

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THE END.

GLOSSARY

OF

FOREIGN AND SCIENTIFIC TERMS.

A.

ABERRATION, in Aſtronomy a ſlight deviation of a Star from the ſtated courſe. Latin.

ABSORPTION, the act of ſucking or ſwallowing up. Latin.

ABSTRACTION, in Philoſophy, that operation of the mind, which purſues a general idea, without attending to the particulars of which it is made up. Thus, *Man*, *Tree*, are *abſtract* ideas, and may be purſued, without deſcending to any one individual perſon or plant, included in the general term. Accordingly, all qualities, ſuch as *whiteneſs*, *cruelty*, *generoſity*, are *abſtract* ideas. Latin.

ACCORD, in Muſic, the ſame with *concord*, the relation of two ſounds which are always agreeable to the ear, whether emitted at once or in ſucceſſion. Latin.

ACHROMATIC-Glaſſes, in Optics, are a recent improvement of the Teleſcope, by which the inconveniency ariſing from the confuſed tranſmiſſion of different coloured rays is prevented. The word is of Greek extraction, and ſignifies *colourleſs*.

AERIFORM, Latin, having the form or conſiſt[illegible] Air.

AEROSTATION, the art of aſcendi[illegible] to the Atm[illegible] means of a Balloon filled with [illegible] Air.

AFFIRMATIVE-propoſition, in Logic, a propoſition which aſſerts or affirms; as *Man is mortal.* Latin.

AIR-PUMP, a machine for making experiments on air, chiefly by exhauſting cloſe veſſels of that fluid.

ALGEBRA, an Arabic word; it is the Science of univerſal Arithmetic; the general proceſs of which is, by comparing ſuppoſed and unknown numbers or quantities, with ſuch as are known, to reduce ſuppoſition into certainty.

ALKALI, an Egyptian word; in Chemiſtry, any ſubſtance which, when blended with an acid, excites fermentation.

ALTITUDE, in Aſtronomy, the height of a Star above the Horizon. Latin.

AMALGAMATE, to incorporate mercury, or quickſilver, with other metals; ſometimes uſed to denote, in general, the mixture and conſolidation of ſeveral ſubſtances, ſo as to make them appear one. Greek.

ANALAGOUS, having reſemblance or agreement. Greek.

ANALYSIS, reſolution into firſt principles, whether in grammar, logic, mathematics, or chemiſtry. To give an inſtance, in Grammar, which is perhaps the moſt familiar to a young perſon's mind, an *analyſis* of this ſentence; *In the beginning God created the Heavens and the Earth*, is to indicate the various parts of ſpeech of which it is compoſed, and the grammatical rules according to which they are arranged. The child may innocently amuſe himſelf with the idea of a *Chemical analyſis*, by a ſcientific proceſs applied to a bowl of the vulgar liquor called punch. To analyze it, is to aſcertain, by ſeparation, the proportion of ſugar, lemon-juice, ſpirit, and water, of which it is compoſed. Greek.

ANATHEMA, and it's compounds; ſomething ſet apart to a ſacred uſe; generally underſtood in an ungracious ſenſe; devoted to deſtruction, accurſed. Greek.

ANATOMY, the ſcience of the ſtructure of the body, and the art of diſſecting and reaſoning upon it. Greek.

Angle, the meeting of two lines in a point, but ſo as not to form of both one ſtraight line. Latin.

Antecedent, in Logic, the former of two propoſitions, in a ſpecies of reaſoning, which, without the intervention of any middle propoſition, leads directly to a fair concluſion; and this concluſion is termed the *Conſequent*. Thus *I reflect; therefore I exiſt*. *I reflect* is the *Antecedent*, *therefore I exiſt* is the *Conſequent*. Latin.

Antipodes, the Inhabitants of the Globe diametrically oppoſite to us, and whoſe feet preſent exactly to our feet. Greek.

Aperture, opening. Latin.

Approximation, a coming nearer to. In Aſtronomy, the gradual approach of two ſtars toward each other: in Arithmetic, a nearer approach to a number or root ſought, without the poſſibility of arriving at it exactly. Latin.

Aqueduct, what conveys, or conducts *water*; a pipe, a canal. Latin.

Aqueous, watery, conſiſting of water. Latin.

Arithmetic, the ſcience of numbers. Greek.

Astronomy, the ſcience of the heavenly bodies. Greek.

Astrology, the pretended ſcience of predicting future events by means of the ſtars. Greek.

Atmosphere, the body of air which ſurrounds the globe on all ſides. Greek.

Axis, in Geography, an imaginary ſtraight line paſſing through the centre of the Earth from pole to pole, round which the Globe revolves once every twenty-four hours. Latin.

B.

Barometer, an inſtrument of glaſs filled with quickſilver, which indicates the preſſure of the air, and which is in general uſe, as an index of the weather. The word is Greek, and ſignifies *weight-meaſurer*.

BISECT, to cut into two equal parts. Latin.

BITUMINOUS, like to, or confiſting of *bitumen*, a fat, clammy, eaſily inflammable juice, dug out of the earth, or ſcummed off lakes. Amber, pit-coal, and ſulphur, are hard *bitumens*. Latin.

BOMB, a hollow caſt-iron globe, to be thrown from a ſpecies of great gun called "*mortar*," and intended to burſt by the force of gun-powder, at the moment of falling, and to ſcatter deſtruction all around. It is in this Work innocently employed to explain the path of all bodies forcibly thrown through the air, and the effect of gravity in bringing all heavy moving bodies down to the ground. Latin.

BOTANY, the ſcience of Plants; or that part of natural and medicinal hiſtory which has the vegetable world for it's object. Greek.

C.

CAMERA OBSCURA, an apartment darkened, all but a ſmall circular opening, to which a double-convex glaſs is fitted, and by which external objects are repreſented in their natural colours, motions, and proportions, on a white table within the apartment. Latin.

CATARACT, a body of water precipitated from a great height. Greek.

CATOPTRICKS, that branch of the ſcience of viſion which relates to reflected light. All bodies which you do not *ſee through*, but which from their poliſh and brightneſs *throw back* the light, belong to *catoptricks*, ſuch as mirrors of every kind. The word is Greek, and ſignifies *backward viſion*.

CAVITY, a hollow. Latin.

CAUSA-SUFFICIENS, *ſufficient or ſatisfying cauſe or reaſon*, a jargon employed by certain Metaphyſicians of the laſt age, who attempted to check all rational, experimental enquiry, by calling continually for the *cauſa ſufficiens*, the *adequate cauſe* of every fact that occurred, and of every obſervation that was made; while they were bewildering themſelves, and attempting to bewilder mankind, in a philoſophic maze, uſeleſs, reaſonleſs, and therefore unſatisfactory.

CENTRE, a point within a circle or ſphere, equally diſtant from every point of the ſurface or circumference. Latin.

CHART, a delineation on paper of part of the land or of the ſea, or both. Latin.

CHEMISTRY, or CHYMISTRY, a word of uncertain derivation; the ſcience of ſeparating compound ſolid bodies, or of compounding ſimple bodies into one maſs, by the force of fire.

CHIMERA, a vain and wild imagination. Latin.

CHORAL-muſic, a ſacred band compoſed of voices and inſtruments. Latin.

CHROMATIC; in Opticks, relating to colour; in Muſic, to a certain ſeries of ſounds. Greek.

CIRCLE, a round figure, having this eſſential property, that every point of it's ſurrounding line, called the circumference, ſhall be equally diſtant from it's middle point, called the centre. Latin.

CIRCUMAMBIENT, encompaſſing round and round: applied particularly to air and water. Latin.

CIRCUMFERENCE, the ſurrounding line of a circle, every point of which muſt have an equal diſtance from it's middle point or centre. Latin.

COHESION, clinging or ſticking together. Latin.

COLLISION, the claſhing of one ſolid body againſt another. Latin.

COMET, a ſtar with a fiery train, like flowing hair, averted from the ſun; of uncertain appearance and re-appearance, but undoubtedly forming part of our ſolar ſyſtem. Greek.

COMPLEX, made up of various qualities or ingredients. A beautiful, wiſe and good woman, is a *complex* idea, containing three diſtinct ideas, beauty, wiſdom, goodneſs; you might render it ſtill more complex by the addition of high-born, rich, religious——but I muſt not make my idea too complex.

Compression, the act of reducing to a ſmaller ſpace by preſſure.

Concave, the hollowed ſurface of a globular body. Latin.

Concussion, mutual ſhock, by violent meeting of two ſolid bodies. Latin.

Condensation, the act of contracting air into a ſmaller ſpace, and thereby increaſing it's elaſtic force, that is, it's power of burſting out. Latin.

Congelation, the reduction of a fluid to a ſolid ſubſtance, as water to ice by cold. Latin.

Concentric Circles, one within another, having a common centre. Latin.

Conical, having the form of a *cone*, which is a figure produced by turning round a right-angled triangle about it's perpendicular ſide; a common candle extinguiſher conveys the idea of it. Greek.

Consequent. Take the trouble to look to the word *Antecedent*. They are what is called *correlative*, in other words the one cannot be underſtood but by referring to the other.

Consonance, in Muſic, the agreement of two ſounds emitted at the ſame time. Latin.

Constituent, contributing to make up or compoſe. Thus the conſtituent parts of gun-powder are ſaltpetre, ſulphur, and charcoal. Latin.

Continuity, uninterrupted connection; the unviolated union of the parts of an animal body. Latin.

Contexture, an interweaving. Latin.

Contour, the extreme bounding line of any object. Children delineate the *contours* of each other's faces by tracing, with a pencil, the line deſcribed on the wall, when the face is placed between a light and the wall. It is a French word.

Divergent, ſtraight lines, gradually removing farther and farther from each other. See *Convergent*. Latin.

Diving-bell, a machine of wood, glaſs, or metal, in form of a bell, for the purpoſe of conducting down into the water, with ſafety, perſons employed in certain kinds of fiſhery, and in recovering goods loſt by ſhipwreck.

Divisibility, capability of being divided. Latin.

Double-concave, an optical glaſs which has both ſurfaces hollowed.

Double-convex, an optical glaſs which has both ſurfaces raiſed.

Ducat, a *ducal* coin of gold, current on the continent, value about nine ſhillings and ſixpence.

Ductile, pliant, eaſily drawn or ſpread out. Latin.

E.

Effulgence, luſtre, brightneſs. Latin.

Elasticity, a power in bodies of recovering their former ſituation, as ſoon as the force is removed, which had changed it. Thus, the extremities of a bow are brought nearer by drawing the ſtring, but when the ſtring is relaxed, the bow, by it's *elaſticity*, is reſtored to it's natural ſtate. It is a property of air, as well as of ſolid bodies. Greek.

Electricity, the diſpoſition which certain bodies have of acquiring, by rubbing, the quality of attracting other bodies, and of emitting ſparks of fire. It is derived from a Latin word ſignifying *amber*, which is one of the ſubſtances endowed with the electrical virtue.

Elicite, to ſtrike out by force. Thus by a ſharp ſtroke of the ſteel on flint, fire is *elicited*. Latin.

Elogium, or **Eulogium**, an oration in praiſe of one abſent or dead. Greek.

D.

Decompose, to ſeparate things compounded: thus, in printing, to *compoſe* is to arrange the types in a frame, in the order of words and ſentences; and to *decompoſe* is to take the frame to pieces. Latin.

Degree, in Geography, the three hundredth and ſixtieth part of the circumference of the globe: it contains about 69 Engliſh miles. French.

Density, thickneſs. Latin.

Dephlogistic, deprived of fiery inflammable qualities. Greek.

Detonation, the thunder-like noiſe produced by firing off heavy artillery. Latin.

Diagram, a figure delineated for the purpoſe of mathematical demonſtration. Greek.

Diameter, a ſtraight line drawn through the centre of a circle or globe, from circumference to circumference. Greek.

Diaphonous body, that which eaſily tranſmits the light, as glaſs. Greek.

Diaphragm, in optical inſtruments, a circular piece of paſteboard, or other non-tranſparent ſubſtance, applied to the object-glaſs, to exclude part of the rays of light. Greek,

Diatonic, an epithet given to the common muſic, as it proceeds by tones, both aſcending and deſcending. Greek.

Dilate, to expand, to ſpread over greater ſpace. Latin.

Dimension, meaſure. Latin.

Dioptricks, that branch of the ſcience of viſion which relates to the tranſmiſſion of the rays of light *through* tranſparent bodies. Greek.

Dissonance, in muſic, ſounds that do not harmonize, but are harſh and diſagreeable to the ear. Latin.

Distraction, tendency in different directions. Latin.

Divergent, ſtraight lines, gradually removing farther and farther from each other. See *Convergent*. Latin.

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Formule, a ſet or preſcribed ſtandard: a ſcheme for ſolving mathematical and algebraical queſtions. Latin.

Forte, in Muſic, forcibly, in oppoſition to *piano*, ſoftly. Latin.

Fourth, in Muſic, one of the harmonic intervals, and the fourth in reſpect of agreeableneſs to the ear. It conſiſts of two ſounds blended, in the proportion of 4 to 3; that is, of ſounds produced by chords, whoſe lengths are in the proportion of 4 to 3. See Vol. I. Letters VI. and VII.

Friction, the act of rubbing one ſolid body againſt another. Latin.

Fusible, that may be eaſily melted. Latin.

G.

Gamut, the ſcale of muſical notes. Italian.

Genus, kind, general claſs containing ſeveral ſpecies, which again contains many individuals. Thus, *Dog* is the *genus*; *Greyhound* is the *ſpecies*, and *light-foot* the individual. The Latin plural *genera* is in uſe.

Geography, a deſcription of the Globe. Greek.

Geometry, the ſcience of quantity, magnitude or extenſion abſtractly conſidered. Greek.

Glaucous, azure-coloured. Greek.

Globule, ſmall globe; little particles of a ſpherical form. Latin.

Gradation, regular progreſs from one ſtep to another. Latin.

Gravity, weight; in the ſyſtem of the Univerſe, that principle in all bodies which preſſes them down to, or attracts them toward, their centre. Latin.

Groove, a channel cut out in a hard body with a tool, fitted to another body which is deſigned to move in it.

H.

HARMONY, in Muſic, a combination of ſounds perfectly adapted to each other, ſo as to produce a pleaſing effect on the ear. Greek.

HEMISPHERE, one half of a Globe. Greek.

HETEROGENEOUS, made up of diſſimilar or diſcordant parts; it is the oppoſite of *homogeneous*, which ſignifies, made up of things ſimilar. Greek.

HORIZON, the line which terminates the view. In Geography, an imaginary circle encompaſſing the Globe, and dividing it into the upper and under Hemiſpheres. To a perſon placed at either of the Poles, the *Equator* would be the real *Horizon*. The ſenſible *Horizon* is the circle viſibly ſurrounding us, where the ſky and the earth meet. Greek.

HUMIDITY, moiſture. Latin.

HYDROGRAPHY, a deſcription of that part of our Globe which conſiſts of water.

HYPOTHESIS, a propoſition or doctrine ſuppoſed to be true, but not yet confirmed by irreſiſtible argument or ſatisfying experiment. Greek.

I.

IDEALIST, a kind of Philoſopher who denies the exiſtence of matter, and reduces every thing to *idea* or mental image. Greek.

ILLIMITABLE, what admits of no bound. Latin.

ILLUMINATION, the act of diffuſing light. Latin.

ILLUSION, what deceives by a falſe appearance. Latin.

IMMATERIAL, in Philoſophy, not conſiſting of body or *matter*. Latin.

IMMERSION, in Aſtronomy, the diſappearance of a ſtar by a near approach to the Sun, or the interception of it's light by another ſtar interpoſing between the Sun and it. Latin.

FORMULE, a ſet or preſcribed ſtandard: a ſcheme for ſolving mathematical and algebraical queſtions. Latin.

FORTE, in Muſic, forcibly, in oppoſition to *piano*, ſoftly. Latin.

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FRICTION, the act of rubbing one ſolid body againſt another. Latin.

FUSIBLE, that may be eaſily melted. Latin.

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GLOBULE, ſmall globe; little particles of a ſpherical form. Latin.

GRADATION, regular progreſs from one ſtep to another. Latin.

GRAVITY, weight; in the ſyſtem of the Univerſe, that principle in all bodies which preſſes them down to, or attracts them toward, their centre. Latin.

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IMMERSION, in Aſtronomy, the diſappearance of a ſtar by a near approach to the Sun, or the interception of it's light by another ſtar interpoſing between the Sun [illegible] it. Latin.

Impenetrability, that property of all bodies, in virtue of which no two can occupy the ſame ſpace, at the ſame time. Latin.

Impulsion, the agency of one body in motion upon another. Latin.

Imputability, the quality of being charged upon, or aſcribed unto. Latin.

Incidence, the direction in which one body falls upon or ſtrikes another: and the angle formed by that line, and the plane ſtruck upon, is called the angle of *incidence*. Latin.

Index, the fore-finger, any inſtrument that points out or *indicates*. Latin.

Individual, one ſeparate, diſtinct, undivided whole.

Inertia, that quality of bodies in virtue of which they are diſpoſed to continue in a ſtate of reſt, when at reſt, or of motion, when in motion; and which can be overcome only by a power not in body itſelf. Latin.

Infinity, boundleſſneſs, applied equally to ſpace, number, and duration: *in infinitum* without limit, without end. Latin.

Inflection, the act of bending or turning. Latin.

Inherent, naturally belonging to, and inſeparable from. Latin.

Intellectual, relating to the underſtanding, mental. Latin.

Intensity, the ſtate of being ſtretched, heightened, affected to a very high degree. Latin.

Interception, the cutting off or obſtruction of communication. Latin.

Intersect, mutually to cut or divide. Latin.

Interstice, the ſpace between one thing and another.

Inverse, having changed places, indirect, turned upſide down. Latin.

Iris, the circle round the pupil of the eye. Latin.

L.

Labyrinth, maze, inextricable difficulty or perplexity. Latin.

Latitude, in Geography, meaſurement of the Globe from the *Equator*, Northward and Southward to the *Poles*, in degrees and minutes. The degree contains about 69 Engliſh miles, and a minute is the ſixtieth part of a degree. The higheſt poſſible degree of *Latitude* is at the *Poles*, each being 90 degrees from the *Equator*. Latin.

Lens, a round glaſs, double-convex, for aſſiſting viſion, or deriving fire from the collected rays of the Sun. *Lenticular*, having the form of a *lens*.

Level, riſing or falling to the ſame height. Saxon.

Literati, the learned; the plural of the Latin word *literatus* a learned man.

Logic, the art of right reaſoning, for the purpoſe of inveſtigating, and communicating uſeful truth. Greek.

Longitude, in Geography, meaſurement of the Globe in degrees and minutes, round and round, from Eaſt to Weſt. Latin.

Lunar-tide, the flowing and ebbing of the tide relatively to the Moon. Latin.

Lymphatic, veſſels, ſlender, tranſparent tubes through which *lymph*, or a clear colourleſs fluid is conveyed.

M.

Magnet, and it's compounds, a ſtone which attracts iron and ſteel; and gives a northerly direction to the needle of the Mariner's Compaſs. Art has been enabled, by means of bars of ſteel, ſucceſsfully to imitate the natural *magnet* or loadſtone. Latin.

Magnitude, great[illegible] bulk, extenſion. Latin.

Manichean,

Observatory, an edifice reared for the purpoſe of aſtronomical *obſervations*. Latin.

Occult, ſecret, unknown, undiſcoverable. Latin.

Octave, in Muſic, a regular ſucceſſion of notes from one to eight; the firſt and the eighth having the ſame name and emitting the ſame ſound. Latin.

Ocular-lens, in Optics, that glaſs of a teleſcope which is applied to the eye.

Opaque, what does not tranſmit the rays of light, not tranſparent. Latin.

Optics, the Science of the nature and laws of viſion, or ſight. Greek.

Orb, ſphere, heavenly globular body. Latin.

Orbit, the circular path in which a planet moves round the Sun. Latin.

Oscillation, alternate moving backward and forward like the pendulum of a clock. Latin.

P.

Paradox, a tenet which exceeds or contradicts received opinion, affirmation contrary to appearance. Greek.

Parallel-lines, in Geometry, lines which through the whole of their length maintain the ſame diſtance: they are the oppoſite of *convergent* and *divergent*. Greek.

Parallelism, ſtate of being parallel.

Parallelogram, a geometrical figure of four ſides, having this property, that the oppoſite ſides are equal and parallel, and the oppoſite angles equal. Greek.

Pellucid, what tranſmits the rays of light: tranſparent. Latin.

Pendulum, a heavy body ſuſpended, ſo as to ſwing backward and forward without obſtruction, for the purpoſe of meaſuring time;

MICROSCOPE, an optical inſtrument, which, by means of a greatly-magnifying glaſs, renders diſtinctly viſible objects too minute for the unaſſiſted eye. Greek.

MINOR, in Logic, the ſecond, or particular propoſition of a ſyllogiſm: for example, in this Syllogiſm,

All men are mortal:
But, The King is a man;
Therefore, The King is mortal.

The firſt propoſition, *All men are mortal* is the *Major*; the ſecond, *the King is a man*, is the *Minor*, and theſe two are called the *premiſes*; the third, *the King is mortal*, is the concluſion. Latin.

MOBILITY, eaſineſs of being moved. Latin.

MODE, in Logic, particular form or ſtructure of argument. Latin.

MONAD, a minute particle of matter which admits of no farther ſubdiviſion. Greek.

MONOCHORD, a muſical inſtrument of one ſtring. Greek.

MYOPS, ſhort-ſighted. Greek.

N.

NADIR, the Point in the heavens directly under foot. Arabic.

NAVIGATION, the art of ſailing. Latin.

NEGATION, denial, the oppoſite of *affirmation*. Latin.

NOTION, thought; repreſentation of any thing formed by the mind. Latin.

O.

OBJECTIVE-lens, in Optics, that glaſs of a teleſcope which is turned to the *object* or thing looked at. Latin.

not direct, not perpendicular, not parallel. Latin.

Observatory, an edifice reared for the purpoſe of aſtronomical *obſervations*. Latin.

Occult, ſecret, unknown, undiſcoverable. Latin.

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time: the great perfection of ſuch an inſtrument is, that every vibration or ſwing ſhall be performed in exactly the ſame quantity of time. Latin.

Perception, the power of perceiving, knowledge, conſciouſneſs. Latin.

Permeable, what may be paſſed through. Latin.

Perpendicular, in Geometry, one line ſtanding on another, or on a horizontal plane, without the ſlighteſt inclination to one ſide more than another, and forming right angles with the horizontal line or plane. Latin.

Phalanx, a military force cloſely embodied. Latin.

Phasis, appearance preſented by the changes of a heavenly body, particularly thoſe of the Moon. The Greek and Latin plural *phaſes* is adopted in our language.

Phenomenon, ſtriking appearance of Nature. The Greek plural *phenomena* is in common uſe.

Philosophy, knowledge natural or moral: Syſtem in correſpondence to which important truths are explained: Academical courſe of Science. Greek.

Physics, the Science of Nature; natural Philoſophy. Greek.

Piano, in Muſic, ſoftly, delicately, oppoſite to fortè. Italian.

Piston, the moveable circular ſubſtance fitted to the cavity of a tube, ſuch as a pump or ſyringe, for the purpoſe of ſuction, expulſion, or condenſing of fluids. French.

Planet, a wandering ſtar; thoſe heavenly bodies, our globe being one, which perform a regularly irregular courſe round the Sun, are called *planets*. Greek.

Plano-concave, in Optics, a glaſs which has one ſurface plane, and the other hollow. Latin.

Plano-convex, an optical glaſs which has one ſurface plane, and the other raiſed. Latin.

Plenum, ſpace filled with ſubſtance. Latin.

through another, as from air through water or glaſs. Latin.

Refrangibility, diſpoſition to leave the direct courſe, capability of being broken or refracted. Latin.

Refringent-medium, that which alters, or breaks off, the courſe of rays. Latin.

Reminiscence, the power of recollection, memory. Latin.

Repulsion, the act or power of driving back. Latin.

Resinous, conſiſting of, or ſimilar to reſin, the fat, ſulphureous emanation from certain vegetables. Latin.

Resonance, ſound repeated. Latin.

Respiration, the act of breathing. Latin.

Reticulated, formed like a net. Latin.

Retina, the delicate, net-like membrane at the bottom of the eye, on which are painted the images of the objects which we contemplate. Latin.

Retrograde, moving in a backward direction. Latin.

Reverberation, the act of beating or driving back. Latin.

Revery, looſe, wild, irregular meditation.

S.

Satellite, an inferior, attendant planet revolving round a greater. Latin.

Scalpel, a chirurgical inſtrument uſed for ſcraping a bone. Latin.

Science, knowledge: in the plural, the ſeven liberal arts, namely, Grammar, Rhetoric, Logic, Arithmetic, Muſic, Geometry, Aſtronomy.

Segment, in Geometry, part of a circle formed by a ſtraight line drawn from one extremity of any arch to the other, and the part of the circumference which conſtitutes that arch. The ſtraight line is denominated the chord of the arch, from it's reſemblance to a bow-ſtring. Latin.

Pyrrhonist, an univerſal doubter and unbeliever; derived from the name of the Man.

Q.

Quadrant, the fourth part of a circle; an inſtrument of that form, contrived to meaſure and aſcertain Latitude. Latin.

Quadrilateral, conſiſting of four ſides. Latin.

Quotient, in Arithmetic, the number reſulting from the diviſion of two numbers which meaſure each other. Thus on dividing 36 by 4, we have a *quotient* of 9.

R.

Radius, in Engliſh *ray*, a ſtraight line drawn from the centre of a circle or ſphere to the circumference. The Latin plural *radii* is in uſe.

Rarefaction, the rendering of a ſubſtance thinner, more tranſparent, it is the oppoſite of condenſation. Latin.

Ratio, proportion. Latin.

Ratiocination, a proceſs of reaſoning, a deduction of fair concluſions from admitted premiſes. Latin.

Recipient, that which receives and contains. Latin.

Reciprocally, mutually, interchangeably. Latin.

Rectangular, containing one or more right angles. A right angle conſiſts of 90 degrees. Latin.

Rectilinear, conſiſting of ſtraight lines. Latin.

Reflection, in Catoptricks, the ſending back of the rays of light from an illuminated body. Latin.

Reflux, the ebbing, or flowing back, of the tide. Latin.

Refraction, in Dioptricks, the deviation or broken off courſe of a ray of light on paſſing obliquely from one medium

through

Subterfuge, mean, paltry eſcape or evaſion. Latin.

Subterraneous, under the ſurface of the ground. Latin.

Subtile, thin, not denſe, not groſs. Latin.

Superficial, external, extended along the ſurface. Latin.

Supernatural, what is above or beyond the powers of *Nature*. Latin.

Surface, in Geometry, length and breadth without thickneſs. French.

Syllogism, in Logic, an argument conſiſting of three propoſitions: for example, *Every virtue is commendable: honeſty is a virtue; therefore honeſty is commendable.* See *Major* and *Minor*. Greek.

System, a ſcheme of combination and arrangement, which reduces many things to a regular connection, dependance and co-operation. Greek.

T.

Tangent, in Geometry, a ſtraight line touching a circle externally in a ſingle point. Latin.

Telescope, an optical inſtrument deſigned, by the magnifying power of glaſſes, to repreſent diſtant bodies as much nearer. Greek.

Temperament, ſtate of body or mind as produced by, or depending upon, the predominancy of a particular quality. Latin.

Tension, the ſtate of being ſtretched out, wound up, diſtended. Latin.

Tenuity, thinneſs, delicate fineneſs. Latin.

Term, deſcriptive name, or phraſe; component part, condition. Latin.

Terraqueous, conſiſting of land and water. Latin.

Theology,

Semicircle, the half of a circle; the ſegment formed by a diameter as the chord, and one half the circumference as the arch. Latin.

Semitone, in Muſic, half a tone, the leaſt of all intervals admitted into modern muſic. The *ſemitone major* is the difference between the *greater third* and the fourth, it's relation is as 15 to 16: the *ſemitone minor* is the difference of the *greater third* and the *leſſer third*, and it's relation is as 24 to 25. Latin.

Sensation, perception by means of the ſenſes. Latin.

Series, regular, ſettled, proportional order or progreſſion, as, in numbers, 9, 18, 27, 36, 45, 54, 63 are in a *ſeries*. The word is the ſame ſingular and plural. Latin.

Seventh, in Muſic, the inverted diſcordant interval of the *Second*, called by the Ancients *Heptachordon*, becauſe it is formed of ſeven ſounds. There are four ſorts of the *ſeventh*, of which the following are the proportions in numbers; as 5 to 9: as 8 to 15; as 75 to 128; as 81 to 160: it is harſh and unharmonious.

Solar-tide, the flux and reflux of the tide relatively to the Sun. Latin.

Solution, demonſtration, clearing up of intricacy or difficulty. Latin.

Sonorous, emitting loud or ſhrill ſounds. Latin.

Species, kind, ſort, claſs: ſee *Genus*. It is the ſame in ſingular and plural. Latin.

Spectrum, an image; a viſible form. Latin.

Sphere, Globe. Greek.

Spheroid, approaching to the form of a ſphere, but ſomewhat lengthened. Greek.

Spiritual, not conſiſting of, diſtinct from matter or body. Latin.

Subli place; in Chymiſtry, raiſed by the force of

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Term, deſcriptive name, or phraſe; component part, condition. Latin.

Terraqueous, conſiſting of land and water. Latin.

Theology, ſyſtematic Divinity. Greek.

Theorem, a propoſition announced for demonſtration. Greek.

Theory, a doctrine contemplated and conceived in the mind, but not yet confirmed by irreſiſtible argument, or ſatisfying experiment. Greek.

Thermometer, an inſtrument contrived to meaſure the heat of the air or other body by means of the riſing or falling of a ſpirituous fluid. Greek.

Third, in Muſic, the firſt of the two imperfect concords, ſo called becauſe it's interval is always compoſed of two degrees or of three diatonic ſounds. The *tierce major* or *greater third*, is repreſented in numbers by the ratio of 4 to 5: and the *leſſer*, by the relation of 5 to 6. See Vol. II. Let. VI. and VII.

Tide, the alternate riſing and falling of the Water in rivers, and along the ſhores of the Sea. Saxon and Dutch.

Tone, in Muſic, the degree of elevation which the voice aſſumes, and to which inſtruments are adapted, in order to the harmonious execution of a muſical compoſition: a pitch pipe. Latin and Greek.

Transit, in Aſtronomy, the paſſing of one heavenly body over the diſk of another. Latin.

Transmission, permiſſion to paſs through. Latin.

Transparent, clear, what may be ſeen through, as air, water, glaſs. Latin.

Transverse, in a croſs direction. Latin.

Triangle, a geometrical figure conſiſting of three ſides and three angles. Latin.

Tube, a pipe; a long hollow body. Latin.

Tunicle, a ſmall coat or covering. Latin.

V.

Vacuum, empty ſpace. Latin.

Valve, a moveable membrane in the veſſels of an animal body, and imitated by art in the conſtruction of various machines, which opens for giving paſſage to fluids in one direction, but ſhuts to oppoſe their return through the ſame paſſage. Latin.

Velocity, ſpeed, ſwiftneſs of motion. Latin.

Vertical, perpendicularly over head. Vertical angles, in Geometry, are thoſe formed by the interſection of two ſtraight lines, in whatever direction, making four in all at the point of interſection, and of which the mutual two and two are equal. Latin.

Vibration, rapid, tremulous motion backward and forward. Latin.

Visual, relating to viſion or ſight; belonging to the eye. Latin.

Vitreous, compoſed of, or reſembling glaſs. Latin.

Vivid, lively, briſk, ſprightly. Latin.

U.

Ultimate, final, beyond which there is no farther progreſs. Latin.

Unison, emiſſion of the ſame or harmonious ſounds. Latin.

Untenable, what cannot be maintained or ſupported.

W.

Waning, gradual diminution of apparent magnitude and light. Saxon.

Waxing, gradual increaſe of apparent magnitude and light; particularly of the Moon. Saxon and Daniſh.

Wind-gun, a gun which forcibly emits a ball by means of compreſſed air or wind.

Z.

Zenith, the point in the heavens directly perpendicular or vertical: exactly over head: the oppoſite of *Nadir*. Arabic.

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